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SWIDDEN AGRICULTURE AND THE RISE OF MAYA CIVILIZATION[1]

D. E. DUMOND

IT IS A COMMONPLACE to assert that one precondition for the rise of civilization is a degree of efficiency in the production of foodstuffs, whether stated in terms of economic surplus or of the harnessing of energy. This precondition customarily has been met through some system of agriculture, and it seems frequently to be assumed that the more intensive the agriculture, the more likely it is to produce the requisite surplus. Conversely it has also been argued that civilization has not developed where intensive agriculture did not exist—specifically in cases in which the only support was some form of shifting cultivation.[2] It is this thesis which will be briefly examined here, with special reference to the lowland Maya.

One approach is that of Meggers, who postulated a law to the effect that "the level to which a culture can develop is dependent upon the agricultural potentiality of the environment it occupies."[3] The tropical forest, doomed by natural conditions to be tilled by some method of shifting cultivation, is said thereby to possess a low absolute ceiling for cultural development. She includes the lowland Maya area within this land of little opportunity, and concludes that the lowland Maya

1 Thanks are due R. D. Gastil, T. Stern, V. R. Dorjahn, and L. S. Cressman, who kindly read and commented upon a version of this paper, the L. S. and D. C. Cressman Prize Essay in Anthropology for 1960-61. None, of course, is responsible for its shortcomings.

2 Civilization as here used refers to societies characterized by the most recognizable of the criteria presented by Childe (1950): the possession of "truly monumental public buildings," to which the concentration of "social surplus," some labor specialization, etc., are assumed to be corollary. Swidden agriculture, slash-and-burn or shifting cultivation, is defined after Pelzer (1945, p. 17), as involving rotation of fields rather than crops; clearing by means of fire; use of human labor only; lack of manuring; use of dibble or hoe; use of short periods of cultivation alternating with long periods of fallow.

3 Meggers, 1954, p. 815.

developed civilized knowledge and skills before their arrival in the Petén, and subsequently spent their most spectacular era in a recessionary decline to which the system of agriculture was causal. Swidden agriculture as a basis for civilized developments is held in similar low esteem by others. Barrau, for instance, indicates it can provide only for subsistence.[4] The FAO staff assert that under such a backward system no concentration of population, and hence no urbanization, is possible.[5] Palerm and Wolf state flatly that "slash and burn agriculture . . . could not provide a stable economic basis for the growth and existence of Maya civilization."[6]

But the objection made is not necessarily based upon a lack of productivity, in terms of the return for an hour's labor. On the contrary, efficient swidden farming is seen by many students to be capable of producing a definite surplus. Although this potentiality seems fairly well established, a short review may be in order. It must be emphasized, however, that data are generally scanty, often unreliable, and that efficiency of shifting cultivation practices varies tremendously.

In Mexico, Lewis indicates that yields per acre from maize swiddens of Tepoztlán farmers average twice as high as those from continuously cropped lands.[7] Kelley and Palerm report that Tajín Totonac swidden farmers as a whole tend to produce a small amount more than they consume.[8] LaFarge and Byers state that the swidden farmers of Jacaltenango, in western Guatemala, raise enough maize to carry on a considerable export business.[9] Figures provided by Villa Rojas suggest that in 1935-36—a better-than-average year—the X-Cacal Maya harvested almost three times their annual requirement of maize, and that without farming to capacity they are able to develop and maintain a surplus sufficient to tide them over two or three lean years.[10] Various other studies indicate that swidden farms in the Mexican tropics are capable of producing a maize surplus of from 20 to 100 percent of subsistence needs.[11]

In temperate Europe the Finno-Ougrian peoples in the nineteenth century are reputed to have obtained from shifting cultivation a yield of rye three to four times as high as that possible from continuously cropped fields.[12]

4 Barrau, 1959, p. 55.

5 FAO staff, 1957, p. 9.

6 Palerm and Wolf, 1957, p. 26; cf. Palerm, 1955, p. 31, Wolf, 1959, p. 77.

7 Lewis, 1955, p. 155f. Productivity per hour of labor *with oxen* in the continuously cropped fields is about fifty percent higher than that without oxen in swiddens. Such a comparison is of course not valid when applied to pre-Columbian Mexico.

8 Kelly and Palerm, 1952, p. 118.

9 LaFarge and Byers, 1931, p. 71.

10 Villa Rojas, 1945, pp. 60, 65.

11 Morley, 1947, p. 154; Emerson and Kempton, 1935, p. 140; Drucker and Heizer, 1960.

12 Clark, 1952, p. 92.

The situation reported from southeast Asia is similar. Conklin suggests that production per man-hour in Hanunóo rice swiddens in the Philippines "compares favorably with labor cost figures for rice production under the best conditions elsewhere in the tropics."[13] Figures presented by Gourou suggest that irrigated rice agriculture in the heavily populated Tonkinese Delta is less than half as productive as the Hanunóo swiddens, in terms of expended man-hours.[14] Among the Land Dayak of Sarawak, Geddes reports that irrigated (swamp) swiddens—presumably of higher fertility than would be unfertilized, continuously cropped wet fields in the same area—may not yield more than good upland swiddens.[15] Halpern cites figures from Laos and other portions of Indo-China which "indicate the possibility that hectare for hectare in any given season swidden cultivation can be more productive" than irrigated rice farming.[16] Gourou also points out that a shift from swiddens to "intensively cultivated, permanent fields does not necessarily carry with it an increase of productivity—not if the available arable area is so spacious as to permit suitable fallows and thus evade the risk of soil exhaustion. Tropical cultivators are aware of this." Consequently, he says, swidden agriculturalists may oppose changes in their agricultural systems on purely material grounds:

> The peoples of some mountains of West Africa (Atacora Mountains, Bauchi Plateau, Mandara Mountains, Adamawa, etc.) learned relatively intensive agricultural techniques, such as artificial terracing and manuring, in response to a crisis: they had retreated into the mountains primarily for defense against external dangers, particularly against the raids of slavers. The establishment of peace and the suppression of slavery allowed these peoples to abandon their mountains and to clear the surrounding plains where arable areas were plentiful. In the new environment they are forgetting these techniques and returning to *ladang* [i.e., swidden farming], in which they find greater productivity. In the same way, all the efforts made between 1920 and 1940 to lead the Moi Rhade (Annamite Cordillera of Indochina) to utilize the plow and to till inundated rice fields have been wasted. . . . They have observed that permanent rice fields, without manure, gave a lesser output per day of work than *ladang*. . . . Still worse, the Vietnamese colonists of Ban Methuot, Darlac, have turned for the first time to *ladang* and have abandoned the ancestral plow.[17]

Leach has thrown further light on the change from extensive to intensive cultivation, with evidence from North Burma:

13 Conklin, 1957, p. 152.
14 Gourou, 1956, p. 342.
15 Geddes, 1954, pp. 65f.
16 Halpern, 1958, p. 33.
17 Gourou, 1956, p. 345.

It is noticeable that in the areas of very low population density where the land was plentiful, shifting cultivation was always the preferred technique; efforts by the government to persuade the population to adopt fixed agriculture was almost completely futile. On the other hand wherever the local population density was high so that land was scarce, terraced agriculture is the standard, traditionally established, preferred technique. There is no cultural difference between the shifting agriculturalists and the terraced field agriculturalists. Precisely the same tribal peoples practice both forms of cultivation. They recognize the two techniques as alternatives and are prepared to discuss the relative merits of both. Terraced agriculture represents much harder work but can be relied upon to produce a moderately good yield. Shifting agriculture on the other hand will produce a very high yield in areas where the forest cover has been thick but a very poor yield in areas of deforestation. There is therefore a perfectly definite economic point at which, from the villager's point of view it becomes advantageous to change from shifting agriculture to terraced agriculture.[18]

Clark indicates that the Danubians of Neolithic Europe probably abandoned their swidden agriculture in favor of more intensive methods of tillage only when the population density was too great to permit the shifting system to operate.[19] This may be of general occurrence. It seems not unlikely, for instance, that in ancient Mesopotamia a similar cause existed for the movement of peoples from the hills where agriculture had begun, down toward the dry Mesopotamian plain itself, about 5000 BC,[20] a movement which seems to have necessitated the first use of irrigation,[21] and perhaps of intensive farming techniques.

Bartlett suggests that "wherever a few colonists of a higher culture have gone into countries of primitive culture, they have largely dropped to the primitive [generally swidden-using] level of agriculture. This is well demonstrated by the history of the colonization of the eastern United States. . . ."[22]

Rather than find swidden farming of doubtful productivity, therefore, it seems reasonable to suggest that for reasons of productivity there is everywhere a tendency to employ patterns of cultivation which rely upon movement of the field when fertility drops from over-cropping, rather than systems of intensive cropping. One might go farther and suggest that such practices of extensive agriculture are normally adhered to until population pressure becomes such that the system ceases to be viable through lack of sufficient land for rotation. Such a tendency toward extensive use would not operate, of course, where soil fertility does not diminish—for whatever reason—after continued cultivation, and might be modified in cases

18 Leach, 1959, pp. 64f.
19 Clark, 1952, pp. 98f.
20 Braidwood, 1960, p. 148.
21 Adams, 1955, p. 9.
22 Bartlett, 1956, p. 709.

where use of draft animals makes intensive but less productive cultivation easier in terms of human effort.[23]

But as indicated previously, a lack of productivity per man-hour does not constitute the grounds for the final censure of swidden agriculture. Most critics condemn swidden systems as "a wasteful method."[24] Leach suggests that most criticisms base themselves on the destruction of timber and the dangers of deforestation and erosion.[25] That it is wasteful of forest cover is to an extent clear. In its better examples, however, it seems generally to be conservative of soil fertility: Popenoe, for example, suggests that swidden agriculture in the valley of the Polochic River in northern Guatemala represents a very conservative practice, with the soil exhibiting comparatively high levels of fertility.[26] Many other students agree with him.[27] The significance of this for the support of civilization is clear. A well-balanced system of shifting cultivation should be capable of indefinite duration.

But criticisms of systems of shifting cultivation as foundations for civilizations go beyond productivity and the waste of resources. Meggers states that the shortcomings (for present purposes) of swidden agriculture are notably two: a relatively large amount of land per capita must be available for agricultural use; and settlements cannot remain permanently in one place.[28] Wolf states that "slash-and-burn cultivation usually implies a scattered population, a population unwilling to pay homage to a center of control."[29] Other critics agree.[30]

That settlements are in some instances forced to move to keep up with swiddens is undeniable. It is apparently documented in the case of the temperate zone Finno-Ougrians cited above, even though in preference to moving, lands might be cultivated as much as forty miles away;[31] it is also strongly suggested for the Danubians.[32] It is the case with modern peoples, as for instance with the Djarais of central Vietnam, who reportedly succeed in wrecking each piece of land they utilize by continued burning of undergrowth, and consequently are forced into periodic moves.[33] It is also true in the case of pioneer agriculturalists like some Iban of

23 Cf. note 7, above.
24 Childe, 1951, p. 64; also, e.g., LaFarge and Byers, 1931, p. 69, and FAO staff, 1957, p. 9.
25 Leach, 1959, p. 64.
26 Popenoe, 1959, p. 76.
27 As, Barrau, 1959, p. 55; Lafont, 1959, p. 56; Meggers, 1957, p. 81.
28 Meggers, 1957, p. 82.
29 Wolf, 1959, p. 77.
30 E.g., FAO staff, 1957.
31 Clark, 1952, p. 96, with references.
32 *Idem*, p. 95.
33 Lafont, 1959.

Sarawak who exhibit a habitual preference for virgin timber, and move in order to find it.[34]

But this pattern is by no means universal. A great proportion of swidden farmers are in sufficient balance with their environment that they remain sedentary.[35] Both Conklin and Carneiro present formulas by means of which the balance between population and land may be calculated, in terms of the number of people which may be supported permanently in a given area.[36] With his, Carneiro calculates that a swidden farming group of low-average efficiency in tropical South America should be able to support nearly five hundred people in a single sedentary settlement, and that the Kuikuru of the Upper Xingú region of Brazil could support a farming population of two thousand people in a single sedentary village, in place of their present 145. These figures are in terms of manioc cultivation with 0.7 acre to a full acre per person per year in cultivation. This compares with the figure furnished by Drucker and Heizer for the amount of maize land per year per capita needed in the La Venta region;[37] but the fallow time is shorter in La Venta, so that it seems likely that even larger sedentary farming towns could be supported there.

If the compulsion to frequent movement of villages is not found a necessary part of much tropical swidden farming, the need for large amounts of land per capita cannot be avoided. It is this need which provides the limiting factor in swidden agriculture—a limit to productivity per area, rather than productivity per man-hour. The total number of people supportable in a given area is smaller; population density is of necessity more sharply limited.

In the case of some extremely low-density populations, areas of population concentration are possible and usual—as, for instance, in the case of nomadic peoples who come together in a periodic throng, and remain together in certain seasons with the throng moving as a unit. Such mobility is lacking in the case of swidden agriculturalists, for the most part; it seems likely that in addition to supporting a population of limited overall density the tendency will be for the population to be dispersed in small towns or villages—at least that portion of the population directly engaged in agricultural pursuits, and during the period in which agricultural labor is necessary.

This is the situation envisioned by Wolf, when he terms such a system "centrifugal."[38] And it is perhaps at this point that the crucial question has been raised.

34 Freeman, 1955.
35 Barrau, 1959, p. 55; Pelzer, 1958, p. 127.
36 Conklin, 1959; Carneiro, 1960.
37 Drucker and Heizer, 1960.
38 Wolf, 1959, p. 60.

It seems clear that the better swidden systems in reasonably adequate areas are capable—in terms of return on labor invested—of producing an amount adequate to support not only ceremonial centers but population centers, and a number of specialists. Emerson and Kempton suggest, for example, that where present agricultural practices in Yucatán and Campeche produce a surplus of about twenty percent over the subsistence needs of the cultivators, the same farming population should be capable of doubling its grain output, "thus making possible the food requirements of a non-productive population four or five times that of the present one" of Mérida and Progreso.[39] To be sure, Palerm argues that such support would be impossible for dispersed farmers to provide under aboriginal conditions, from sheer awkwardness of distribution when transportation depended only upon human carriers.[40] But in spite of this assertion the pre-Columbian city of Mayapán in Yucatán remains an example of a center of concentrated population, traditionally supported for two hundred fifty years in an area for which nobody, so far as I am aware, has suggested the practice of anything but swidden agriculture, with a transportation system depending upon nothing but manpower. Palerm and Wolf state that this particular argument "is deceptive in its superficiality," insofar as it supports the case for development of civilization in swidden farming regions, for "the Northern Maya territory was conquered by war-like groups who settled in nucleated and fortified sites from which they dominated and subjected the rural population. The basic patterns of social, political and military organization, however, were introduced from the Mexican highland."[41] Superficial or not, the example supports the supposition that swidden agriculture and transport by human porters is sufficient to provide for an urban settlement, once it has developed.

The problem remaining, then, concerns the assertion that shifting cultivation is by its nature socially "centrifugal," and hence is inimical to the centralization implicit in any civilization, including the Maya—whether the latter in its Classic manifestations in the Petén involved "urban" concentrations of city dwellers or whether it existed in terms of ceremonial centers supported by a dispersed population. At this point little argument should remain in terms of subsistence economics. The problem is one of social organization.

Kroeber, when discussing the effect of the size of a society on its organization, suggests that urbanism—civilization in the present sense—is more likely in larger societies.[42] Steward argues that high population density without centralization

39 Emerson and Kempton, 1935, p. 140.
40 Palerm, 1955, p. 31.
41 Palerm and Wolf, 1957, pp. 27f.
42 Kroeber, 1948, pp. 272ff.

does not produce cities.[43] That is to say, the size of the society, in Kroeber's terms, is a matter of organization; a large society is possible—theoretically, at least—in an area of dispersed population as well as in an area of population concentration. The question to be raised here is whether a measure of social centralization may be achieved by people who live as dispersed as do swidden farmers.

What population densities are possible under swidden agriculture? Pelzer suggests that up to one hundred thirty per square mile may be supported in the Asiatic tropics without soil damage.[44] Ooi Jin-Bee provides a list of carrying capacities of lands under shifting cultivation in tropical areas, varying from eighteen to one hundred sixty persons per square mile, with more than half the eleven figures above eighty.[45] Some of these involve root crops, but more involve the cultivation of upland rice. One citation refers to a maize producing area, British Honduras, with a figure of sixty per square mile. Drucker and Heizer provide figures for La Venta which indicate that one person could be fed indefinitely by a maize production of 3.75 acres.[46] If somewhat less than sixty percent of the total land area is arable—about the proportion found usable in a study at Uaxactún[47]—such a region would support one hundred per square mile. Emerson and Kempton suggest that with farming methods in practice at the time of their study, Campeche and Yucatán could support sixty per square mile, and that the carrying capacity could be increased without drastic or harmful change in methods.[48] Densities such as those mentioned here, one might point out, are not extremely low.

That populations no more dense than these are capable of centralized organization seems evident from the most superficial consideration of evidence from Africa, a continent of relatively light population, but

> from the point of view of organization and administration, the political acumen . . . in tribe after tribe equals, where it does not surpass, anything in the nonliterate world. Not even the kingdoms of Peru and Mexico could mobilize resources and concentrate power more effectively than could some of these African monarchies, which are more

43 Steward, 1955, pp. 73ff.

44 Pelzer, 1945, pp. 23, 29.

45 Ooi, 1958, p. 113.

46 Drucker and Heizer, 1960, p. 43.

47 Morley, 1947, pp. 313f.

48 Emerson and Kempton, 1935. By way of comparison, Thompson (1954, 29) suggests population figures such that ancient inhabitants of the Petén and Yucatán would not have exceeded one million at their most populous; using Morley's (1947, p. 316) estimate of 50,000 square miles for the habitable (not total) area of the Yucatán peninsula, one arrives at a figure of twenty inhabitants per habitable square mile, or ten per square mile of total area. This seems modest indeed. Morley's more ambitious figure of 13,300,000 inhabitants provides a density of 133 per square mile—probably too high, but no higher than figures from present-day swidden-farming areas of Java, Malaya, and northern Nigeria (Ooi, 1958, p. 113).

to be compared to Europe of the middle ages than referred to the common conception of the "primitive" state.[49]

Although population figures for portions of Africa are notorious for their inaccuracy, at least a rough idea may be gained of certain aboriginal densities. According to figures presented by Forde, estimated census figures for the Yoruba in 1921 were slightly over two million, with those of 1931 running slightly more than three million, an unlikely increase of fifty percent in ten years.[50] Hardly more believable is the increase necessary to achieve the 1952 census figure of more than five million. There are, however, a number of estimates of the size of individual Yoruba settlements, and it seems reasonable that these figures are more apt to be accurate than those for the Yoruba as a whole. Bascom[51] presents figures for a number of large Yoruba settlements which may be compared; summing these for 1911 and for 1952—and excluding figures for the modern city of Lagos—one obtains a suggestion of an increase in these populations of some one hundred percent in the indicated forty years. Bascom's citation of Moloney's estimates of ten city populations of 1890 are higher in total than are the same cities in the 1911 census, perhaps as the result of inaccuracy of estimates in either count, or it may be that in 1890, at the end of a long period of slave wars, the population was more concentrated than it was twenty years later. Allowing for a considerable population decline in the nineteenth century as a result of the wars, or for a modern increase in urbanization, one might rather tentatively suggest an increase of fifty percent in general population between 1850 and 1952. Applying this proportion to figures for population density cited by Bascom from the 1952 census, one obtains an approximate range of 1850 population densities between fifty-five and two hundred forty per square mile.[52] Of these, only the density of Ibadan province (at two hundred forty) exceeds one hundred per square mile, and the city of Ibadan itself, with a population in 1952 of nearly 460,000, is known to have developed principally as a nineteenth century military center.[53] These people are swidden agriculturalists, today raising chiefly yams, maize, bananas, and manioc as subsistence foods.[54] Their penchant for dwelling in big settlements, however, was well

49 Herskovits, 1948, p. 332.

50 Forde, 1951, p. 3.

51 All subsequent Yoruba census figures are from Bascom, 1959, pp. 31-33.

52 In a personal communication, William Bascom generously provided suggestions of means for tentatively estimating 1850 Yoruba population density, emphasizing that all such estimates must remain somewhat questionable for lack of sufficient and reliable data. His assistance is greatly appreciated, but it should go without saying that he can in no way be held responsible for any untoward results.

53 Bascom, 1959, p. 35.

54 Forde, 1951, p. 6.

developed at the time of the first European contact. Such centers are reported from early in the sixteenth century. In the 1850's they were estimated to have at least nine settlements with populations between twenty and seventy thousand, with craft specialization and social stratification.[55]

Less spectacularly, the Azande of Sudan are estimated at 231,000, in figures cited by Baxter and Butt.[56] The Zande district of Sudan, as mapped by de Schlippe, cannot include more than twenty thousand square miles.[57] The density, therefore, should not exceed twelve per square mile. The Azande maintain dispersed settlements, and their traditions and swidden agricultural operation—the latter utilizing millet, maize, sorghum, rice, pulses, and some oil seeds[58]—are such that moves of homesteads are fairly frequent, some of them motivated by soil depletion, but others brought about by other causes.[59] This is an indigenous pattern, yet the indigenous organization was markedly centralized, with a pyramidal organization headed by a king from a hereditary ruling clan, to whom were owed services and tribute. In some areas at least, the number of people under a semi-independent chief, subordinate to a king, might reach or exceed ten thousand.[60]

In sum, although it seems reasonable that shifting cultivation should tend to produce scattered populations, such scattering is apparently not always the result, and even when it does occur it may not preclude the support of a centralized social or political organization. Parenthetically, Kroeber's citation of Mooney's figure for the population density of the federally-bent five Iroquois tribes is less than one per five square miles.[61]

55 Bascom, 1959, pp. 31, 38ff. The Yoruba base much of their subsistence upon root crops, and it seems possible that such crops may produce more heavily than will cereals or maize. Barrau (1959, p. 54), for instance, indicates that in Melanesia generally about one quarter acre of garden suffices to feed one person for one year, and that a total of about two and one half to five acres suffices to feed one person indefinitely. Figured as above, with about sixty percent of the area cultivable, one finds that in Melanesia between about seventy-five and one hundred fifty people could be supported per square mile. It may be noted, however, that anthropologists have not always agreed on the potentiality of root crop cultivation: Palerm and Wolf (1957, p. 28) suggest that large sedentary populations may be supported by root crops in tropical areas more easily than by maize; Meggers (1957, pp. 82f) suggests the reverse—that root crop cultivation is more centrifugal than cultivation of temperate crops, because the lack of a single season of maturation means that farm work is more constant, hence dispersed fields must be tended more frequently. At any rate, with the exception of those for the Ibadan area, the Yoruba densities arrived at above seem in general accord with the carrying capacity of land in the lowland Maya area as estimated by Emerson and Kempton (1935) and others.

56 Baxter and Butt, 1953, p. 13.

57 De Schlippe, 1956, p. 5.

58 *Idem*, pp. 48ff.

59 *Idem*, pp. 191ff.

60 Baxter and Butt, 1953, pp. 48ff.

61 Kroeber, 1939, p. 140.

That some patterns of shifting cultivation do militate against social cohesion in terms of supra-village units, is not denied. This must certainly be the case with peoples who are still "pioneers"—breakers of virgin soil—as in the case of the Iban of Sarawak mentioned above. Here, on rather poor land, is possible a density of thirty-five to forty people per square mile, although present density is much less as the population continues to spread in search of virgin timber, utilizing techniques which are generally destructive of soil.[62] In this area there is still new land to be gained. Here, village long-house communities are the only groups with any cohesion above the nuclear family, having functions in the sphere of religion and the settlement of land claims, but without productive economic role.[63]

The situation is somewhat changed among the Land Dayak of Sarawak, however. Here Geddes reports a heavier population, having little virgin timber land available.[64] When villages multiply through a process of hiving off, daughter villages may continue to pay allegiance to the mother village.[65] And this situation is important to the present argument, for in the maintenance of such ties may be found the seeds of a later centralized development.

Lacking such affective ties, village units might be united by warfare. Herskovits suggests that,

> In all West Africa, the early organization seems to have been that of village autonomy or, at most, the rule of several neighboring settlements by the head of the largest village, so that the number of petty kingdoms which existed before the time of their consolidation into great kingdoms such as Dahomey, Benin, Ashanti, and others, was extensive.[66]

The extent to which warfare was causal, however, is not certain. Bascom, for instance, indicates that "we cannot say either that the early Yoruba cities did or did not develop as defensive or predatory centres, as Ilorin, Abeokuta and Ibadan did in the 19th century. But urbanism clearly antedated the slave trade to the Americas. . . ."[67]

It has been suggested by Willey, based in part on work by the Coes at Nohoch Ek,[68] that even the smallest Pre-classic villages in Mesoamerica may be found eventually to have been constructed around modest ceremonial centers.[69] If the

62 Freeman, 1955, p. 134.
63 *Idem*, pp. 8-10.
64 Geddes, 1954.
65 *Idem*, p. 10.
66 Herskovits, 1938, vol. 2, p. 3.
67 Bascom, 1959, p. 38.
68 Coe and Coe, 1956.
69 Willey, 1957, p. 1.

situation is such that new villages retain some allegiance to the ceremonial centers of parent villages, the seeds of centralization have been sown.

Pre-classic remains are not plentiful in the lowland Maya area, to judge by excavations to date. It seems possible that during Pre-classic times small, more or less autonomous villages spread through the lowland Maya area, perhaps in search of virgin land. As the land was filled, migrations became less frequent, and ties between related villages became stronger, with ceremonial relationships elaborated. From this developed the centralization of Classic times, aided, perhaps, by a little warfare here and there. The result of such a process should be very close to the organization of the Maya as formulated by Willey.[70]

The foregoing should not be construed as an argument that Classic Maya civilization in the lowlands was never supported by anything but swidden agriculture. The question as to what was in fact the basis of subsistence can be answered only from work in the field. Rather, this paper is intended to suggest that the development of a centralized, stratified system such as seems indicated for the Classic Maya is by no means impossible under conditions of shifting cultivation. Indeed, present indications of intensive agriculture in ancient Mesoamerica are not demonstrably as early as the rise of notable ceremonial centers at the beginning of the Classic and earlier. Consequently, on the basis of present evidence and in spite of the argument of Palerm that concentration of resources and major ceremonial centers are impossible under conditions of rainfall agriculture in Mesoamerica,[71] it seems only reasonable to conclude that the development of such concentrations and such centers began under conditions of extensive—as opposed to intensive—agricultural practices, probably largely swidden agriculture. It is further suggested—although a consideration of the question has not been attempted here—that a similar development may have occurred in other areas generally conceded to have supported civilizations, at least in those areas in which rainfall agriculture is possible at all.[72] In areas where feasible, however, the growth of population would shortly have made the practice of some form of intensive agriculture necessary.

That is to say, it seems likely that centralization and urbanization tend to pre-date the appearance of intensive cultivation and of heavy population growth. Although a discussion of the mechanisms involved is again outside the scope of this paper, it seems likely that by increasing the chances of human survival such

70 Willey, 1956.

71 Palerm, 1955, p. 31.

72 Pelzer (1945, p. 20), for example, cites suggestions to the effect that swidden agriculture was basic, or at least extremely important, to other tropical civilizations, specifically that of Angkor, in Cambodia, and that of Anuradapura, in Ceylon.

centralized organization may stand in a causal relation to population growth, rather than vice versa.

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ANALOGY IN ARCHAEOLOGICAL INTERPRETATION

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THE WORK of the archaeologist can be divided into four tasks. First there is the formulation and refinement of concepts; second, data gathering and processing; third, the interpretation of the data, and finally, synthesis. The four tasks are obviously related in an hierarchical scheme: concepts enable meaningful synthesis, synthesis depends on interpretation, and interpretation is ultimately founded on archaeological data.

Substantial progress has been made in approaches to the first, second, and fourth tasks in recent years. Productive work on concepts is illustrated by the successful *Seminars in Archaeology* of the Society for American Archaeology. The appearance of the new journal *Archaeometry* under the auspices of The Research Laboratory at Oxford, with its concentration on the application to archaeology of instruments developed in other disciplines, indicates how vigorous the attack on the second task has been. The ambitious work *World Prehistory* by Grahame Clark, if not wholly successful, demonstrates that a synthesis of human prehistory on a global scale is now feasible. What can be demonstrated for concept formulation, data gathering, and synthesis, cannot be easily shown for archaeological interpretation. If it is granted that acceptance of synthesis must vary with confidence in interpretation, it becomes apparent that interpretation warrants attention.

The most widely used of the tools of archaeological interpretation is analogy. In its most general sense interpreting by analogy is assaying any belief about non-observed behavior by referral to observed behavior which is thought to be relevant. The purpose of this paper is to examine this single interpretative tool. Concentration is on analogies where no historical records are available as aids. Evidence which suggests that there is cause for concern with the present status of analogy as an interpretive tool is presented and some suggestions are sketched.

The introduction of analogy into archaeology can be traced to the era of the classical evolutionary ideology. Analogy in this period was elementary: if it were true that certain living peoples represented early phases of human history, then the interpretation of the remains of extinct peoples could be accomplished by direct reference to their living counterparts. A monument to this logic is Sollas' *Ancient Hunters*. In this work the Tasmanians, Australian Aborigines, Bushmen, and Eskimos were enlisted as modern representatives of four successive paleolithic complexes. The question of the use of any class of palaeolithic tools could be satisfied by direct referral to one of the four groups. For example:

Anthropologists are generally agreed that the Palaeolithic "coup de poing" was not provided with a haft, but was held directly in the hand; and that it was not used simply as a "chopper": it is extremely gratifying therefore to find that the Tasmanians had no notion of hafting their homologue, or rather analogue, of the "coup de poing," and that it served a variety of purposes, among others as an aid in climbing trees.[1]

Interpretation in this mode, however, was not without its anachronisms. It was noted that living representatives of early periods occasionally enjoyed the use of classes of objects which were thought to be distinctive of later periods. In discussing the Australian Aborigines, for example, Sollas noted that polished stone axes ". . . are supposed to be the exclusive characteristic of the Neolithic period; but as the Australians are still in a Palaeolithic stage of culture, they present us in this case with an exception, for which various explanations may be found." In resolving this problem Sollas calculated that they might have invented it themselves or borrowed it from neighbors, but he eventually concluded with the suggestion that the Australian Aborigines learned to polish stone via an extensive network which at one time stretched from Australia to Europe.[2]

The critical reaction to the evolutionary assumptions, coupled with both the unexplained residues resulting from this early approach and the recovery of new data, forced reconsideration. As a result analogy was partitioned, and now at least two broad categories of analogy are recognized.[3]

The first category encompasses the classical evolutionary usage with appropriate shrinkages in the length and breadth of the time and space dimensions. In those areas of the world where history grades into archaeology, or where, in the absence of written documents, analysis of current or recent practices and archaeological data indicate continuity, archaeological data is interpreted by analogy to historical or living groups. In parts of the Near East, for example, archaeological evidence for the process of beer brewing can be interpreted by referral to both ancient texts and contemporary practices. The folk-cultures of Europe exhibit farming tools and practices, structures such as houses and granaries, and devices for transportation, which can be linked directly with the prehistoric past.

What is called the "folk-culture approach" by students of Old World archaeology is paralleled in the New World by the "direct historical approach." Both

1 Sollas, 1911, p. 74.

2 *Idem*, pp. 179, 207-209.

3 A third category has sometimes been distinguished. This third category includes analogies to properties common to all men such as the need for capturing energy and the possession of a language. For purposes of interpretation this third category is meaningless. One does not need to undertake archaeological investigation to know that the individuals in a particular culture engaged in these activities. The question which the archaeologist seeks to answer is what were the particular patterns of a prehistoric people in carrying out these and similar activities.

approaches admit the initiation of study from either end of the time scale. It is legitimate, presumably, to study the historically known prior to close examination of the archaeological unknown, or, reversing the order, to procede from the archaeologically known to the historically unknown. If there is any sutble difference between the Old and New World approaches it is only that the longer time span in the Old World encourages the conception of smooth continuous passage from archaeology into history whereas in the New World the line between the two is more severely drawn.[4]

The withdrawal of the application of analogy from archaeological data where living representatives were assumed, to data where living or documented representatives could be demonstrated, left uncovered a vast temporal and spatial tract for which archaeological data existed. In order to cover this tract, consisting of over ninety-five percent of human history and a large proportion of the globe, a second category of analogy came into use. This second category is here called the new analogy to distinguish it from analogy where historical continuity was assumed, as in the past, or is demonstrated, as in the present.

Anxious to avoid the mistakes of the early evolutionary school, and in the absence of any universal and unique model to guide in the recasting of interpretative tools, the new analogy has been set in a restrained format. In effect, the new analogy consists of boundary conditions for the choice of suitable analogs. A consideration of the canon for the selection of analogs, the qualifications placed on the power of the tool, and an example may characterize the theoretical posture of the new analogy.

According to Clark the archaeologist should ". . . restrict the field of analogy to societies at a common level of subsistence," and should ". . . attach greater significance to analogies drawn from societies existing under ecological conditions which approximate those reconstructed for the prehistoric culture under investigation than those adapted to markedly different environments."[5] Willey would select cultures on ". . . the same general level of technological development, perhaps existing under similar environmental situations."[6] V. Gordon Childe advised that an analog ". . . drawn from the same region or ecological province is likely to give the most reliable hints. . . ."[7] In summary, then, the canon is: seek analogies in cultures which manipulate similar environments in similar ways.

The qualifications on the new analogy are weighty. The mass of archaeological data yields subsistence or subsistence-connected information; hence, relevant anal-

4 Compare Steward, 1942 with Hawkes, 1954.
5 Clark, 1953, p. 355.
6 Tax, *et al.*, 1953, p. 229.
7 Childe, 1956, p. 51.

ogies are to be initially restricted to this domain. The archaeologist is cautioned that the new analogy can provide only ". . . useful clues to general conditions, it can be a dangerous guide to the particular manifestations of culture . . .,"[8] or may ". . . in fact afford only clues in what direction to look for an explanation in the archaeological record itself."[9] The connection between the living culture or cultures and the archaeological culture in question is purely formal; there is no implication of direct generic relationship nor are any dimensions of space and time implied.

The following citation, from the interpretation of the mesolithic site of Star Carr, is an excellent example of the new analogy:

> The character of the finds suggests that we have to deal at Star Carr with a community rather than with the activities of a specialized group. The masculine element is sufficiently emphasized by the importance of hunting and by the evidence of great activity in the manufacture of tools and weapons. On the other hand, to judge from analogy with the hunting peoples of North America and Greenland, the importance of skin-working at Star Carr argues for the presence of women. Among the Eskimos generally women are mainly responsible for flaying the kill and preparing the skins for use. Men certainly play their part, especially in the hard task of thin-scraping caribou skins or when for some magical reason, as in preparing drum-skins among the Caribou Eskimos, it is considered wrong for women to undertake some particular task. Generally, though, it is agreed that the task is predominantly feminine and in fact constitutes the main part of women's labor.[10]

It would be misleading to imply that the restraint advocated in some quarters is practiced wherever archaeological data is interpreted by analogy. In fact, it would not be difficult to cite numerous cases in which less caution in the choice and use of analogs is clear. Consider, for example, the following attempt to interpret the *absence* of the caudal vertebrae of the otherwise well represented bovids in the important Australopithicine sites in the Makapansgat valley.

> To "tail" anything still signifies to "track it down." The leaders of Bushmen hunting parties, when tracking down their prey, signal to one another silently with the bushes or tails of the Cape fox. Tails spontaneously form flexible whips or flagella for beating thickets and grass-lands after game. The flagellum was one of the badges of the Pharaoh! The brush of a fox is the trophy of the chase. The warriors of Predynastic Egypt all wore bushy tails, that look suspiciously like fox-tails, and Pharaohs are delineated on Egyptian monuments retreating from the presence of gods looking back and trailing the bushy tails of an animal behind them. Horse-tails used to be emblems

8 Clark, 1953, p. 355.
9 Childe, 1956, p. 49. See also Clarke, 1951.
10 Clark, 1954, p. 10.

of rank formerly in Turkey, the rank depending on the number of tails (e.g., a pasha of three tails). Every South African witch-doctor carries an animal's brush preferably that of a wildebeste as every European witch carried a broom. It seems likely from the significance attached to tails universally by mankind in myth and history that their disappearance from the Makapansgat breccia is significant; they were all probably in great demand as signals and whips in organized group-hunting outside the cavern.[11]

In the engaging, less extreme example below an attempt is made to interpret the persistence of certain ceramic motifs in northern Georgia, U. S. A. Unlike the previous example, an awareness of boundaries is shown, if not rigorously adhered to.

I am not quite sure to what extent we can measure general ethnic continuity in terms of ceramic continuity. Modern women of our civilization seem much bolder than men in quickly adopting new fashions which seem to display no continuing evolutionary or gradual developmental stages, although these fashions definitely run in cycles. Modern women's status and functions, however, are of course quite different from those of the average southern squaw. Perhaps in the aboriginal Southeast, important new cultural traits that appeared suddenly and are the criteria for many of our major archaeological period designations were exclusively male interests: new weapons, pyramidal mounds, cult paraphernalia, things adopted by conquered or converted men; while the ladies stayed at home and made pottery that changed only gradually as the generations passed. Or perhaps we might better look at our own china dishware to see an expression of conservatism in spite of almost annual changes in foreign policy, Kinsey attitudes, hemlines, and hairdos. Even the atomic age will probably not change our chinaware, except maybe to break more of it.[12]

If the caution of the new analogy did not curb many, it did inhibit others to the point of not undertaking interpretation at all. In 1948 Taylor's *A Study of Archaeology* confronted New World archaeologists with their hesitancy to venture contextual interpretations. What Taylor did not realize was that to some conscientious archaeologists the strictures on interpretation, at least interpretation by analogy, may have in practice appeared formidable. More importantly, one student has argued that the new analogy is ineffectual in important areas, a second that interpretation by analogy is untenable; a third has abandoned hope of making any impartial judgment of the reasonableness of an archaeological interpretation. It will be instructive to consider these three points of view.

Hawkes perceives several kinds of cognition in archaeology. The distinction between them is marked by the degree to which history can be used in the interpretation of archaeological data. The kind of cognition for which the new analogy

11 Dart, 1957, pp. 167-168.
12 Wauchope, 1949, p. 23.

must be employed is ". . . a world wholly anterior to textual-historical evidence." In this world, Hawkes contends, interpretation cannot penetrate much beyond technology and subsistence. It is in these very aspects that man, according to Hawkes, is most similar to other animals. Where man is most unlike other animals, for example, in the possession of social, political, and in particular, religious institutions and systems, interpretative tools are near powerless.[13] An extreme position is taken by Smith: "It used to be thought," Smith writes, "that studies of surviving primitive peoples would provide the necessary analogies for interpreting prehistoric societies; but in the event the extension of ethnological studies has only served to show what an incredible variety of codes of behavior in fact actuate human conduct." Given this diversity, to ask for interpretation which utilizes living groups, is to demand "logical alchemy." Statements resulting from interpretations by analogy are assertions, not arguments, according to Smith. Imagine a situation in which at a given site one house structure is larger than all other house structures. If the larger structure is called an X, and not a Y or a Z, where X, Y, and Z refer to uses of a single large structure in living groups, then "You can't really say that you *know* that it is [an X], and if someone criticizes your assertion, it is impossible to produce sufficient evidence to convince him you are necessarily right." Smith finds interpretation by analogy indefensible and argues for its abandonment.[14] A third position is taken by Thompson. He grants primacy to the role of analogy in interpretation but contends that an evaluation of its use in any particular instance can be made only by assessing the competence of the user. Thompson dismally maintains that there is no way to improve this situation other than hoping for ". . . improvements in the methods of measuring the amount of faith we place in an individual's work."[15]

From the foregoing discussion it is apparent that there is no general agreement on the new analogy, either in theory or practice. Certainly a call to abandonment is sufficient cause for discomfort. If it were not for the fact that analogy in archaeological interpretation has suffered chronic ambiguity since the nadir of classical evolutionary simplicity, an impasse could be said to exist. The following suggestions are sketched to aid in placing analogy on a firmer foundation.

1. For any given archaeological situation there usually exists more than a single analogy which can be used in the interpretation of the data. The real problem is to select from this finite range of possible analogs the one which offers the *best solution*. Selection of the best solution is most efficient when the least satisfying

13 Hawkes, 1954, pp. 161-162.
14 Smith, 1955, pp. 4-6.
15 R. H. Thompson, 1956, pp. 331-332.

solutions are eliminated in a systematic way. Thus, a first elimination may be made on the basis of the economies, a second on the basis of the distances from the archaeological situation to the possible analogs as measured in terms of space, time, and form, and a third elimination may be based on the closeness of fit of the relationships between forms in the archaeological situation with relationships between forms in the hypothesized analogous situations. It may be that archaeologists in seeking analogs work in a systematic manner; but if they do it is seldom evident in the final solutions offered. Consider the following example:

> In this new soil, which was sticky and grey compared to the loose brown material in which the painted pottery had been deposited, we found polished-stone axes, polished-stone chisels, and flint sickle blades shiny from grain gloss. There was a brief alert when we thought we had come upon a burial, but it was a false alarm. Lying side by side in the soil were two large human thighbones, brown and shiny, polished from much handling. As they were completely alone, they were not part of a burial at all. All I could think of to explain their presence was that the ancient inhabitants of the Canary Islands, who were Neolithic people, had consecrated their kings by holding just such a pair of bones over their heads, and that pairs of thighbones were also used in the rituals of some of the Nilotic tribes of the Sudan. Perhaps the kings of Hotu had been similarly initiated into office. Who knows?[16]

If a systematic approach were used (it is not clear whether or not it was used in the above example), and the alternative solutions for a particular situation stated instead of the usual statement of a single solution (as above), there would be no need to examine credentials (which, in the above case, are extraordinary), but only the argument and the result. There is no touch of alchemy in the procedure outlined. Solutions to any problem are at best approximations arrived at by the elimination of those least likely. Simply, what is being suggested is the introduction of a clear systematic approach and considered statements of results in terms of degrees of likelihood.

2. It has been argued that the existing ethnological literature is inadequate for the purposes of archaeological interpretation because it contains either ideal descriptions of technologies, detailed descriptions without behavioral correlates, or no descriptions of technologies. On this basis it has been proposed that the archaeologist turn to the living community to compile his own inventories.[17] There is no question as to the merit of this suggestion.[18] If the argument which leads to the

16 Coon, 1957, p. 186.

17 Kliendienst and Watson, 1956, pp. 76-77.

18 This idea is of course not novel. For an excellent example see D. F. Thompson, 1939. Unfortunately most of the studies of this type have been directed at demonstrating that many aspects of a culture are not preserved in archaeological data.

suggestion is valid, however, then the procedure outlined in section 1 above might be acceptable in theory but not possible in practice. Is the argument valid?

There does exist, as has been emphasized by Kidder and Forde, a rich and suitable literature which is neglected by the archaeologist.[19] The store of information on pottery manufacture and its associated behavior, for example, is copious. A codification of this literature and other similar information banks would be useful. There are, further, at least some quantitative models based on ethnographic data which are available and qualitative models can be designed to fit the needs of the archaeologist.[20] Behavioral interpretation, in terms of degrees of likelihood, beyond subsistence-connected activity, is only apparently remote.

3. The past and the present, it is often claimed, serve each other: archaeology depends on ethnographic data for interpretation; ethnology can make use of temporal depth that studies of the past may provide. This dogma, useful as it may be for certain purposes, has contributed to drawing a fast distinction between the ongoing and the extinct, the living and the dead. It is my contention that no clear distinction exists with regard to the material evidence of culture. The point is not trivial, for the generally assumed polarity between the ongoing and the extinct has resulted in the total neglect of striking relevant data.

Every living community is in the process of continuous change with respect to the materials which it utilizes. At any point in its existence some proportion of materials are falling into disuse and decomposing, while new materials are being added as replacement. In a certain sense a part of every community is becoming, but is not yet, archaeological data. The community becomes archaeological data when replacement ceases. What the archaeologist disturbs is not the remains of a once living community, stopped as it were, at a point in time;[21] what he does interrupt is the process of decomposition. The observational fields of ethnology and archaeology overlaps on that proportion of a living community which is in the process of transformation. It is the study of this very special corpus of data within the living community which holds the most fruitful promise for analogy in archaeological interpretation.

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19 Tax, *et al.*, 1953, pp. 231-232.

20 For examples of the use of both types of models see Ascher, 1959 and 1961.

21 This erroneous notion, often implicit in archaeological literature, might be called the Pompeii Premise.

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HIMALAYAN ROPE SLIDING AND VILLAGE HINDUISM: AN ANALYSIS[1]

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INTRODUCTION

ONE of the most spectacular, yet little-known ceremonies in India is rope sliding, performed in the lower Himalayan area and known variously as *bēdā-rat, barta, badwār, barat*. Until I obtained two eye-witness accounts of this ceremony in western Garhwal, I had been unaware of its existence and, in fact, was at first suspicious that my informants were taking advantage of my credulity. I have since located seven brief, apparently independent accounts by nineteenth century observers which show a minimal distribution of the ceremony from Garhwal District to the Sutlej valley, i.e. on both sides of the border between Central and Western Pahari-speaking areas.[2] The half-dozen other references that I have found all derive from the accounts of Moorcroft and Trebeck, and of Traill,[3] while some of the best accounts have been overlooked. The ceremony has even found its way into a novel by Philip Woodruff.[4]

Rope sliding is mentioned by these authors most often as a quaint, improbable, and fascinating performance. Some have maintained that it is derived from a form of human sacrifice and Frazer cites it in *The Golden Bough* as an example of the use of a human scapegoat. In none of the accounts, however, is there a full description of the ceremony nor an attempt to analyze it in cultural context. In this paper, by briefly considering the nature of the ceremony and its variations, affinities and alterations, its status as a Hindu ceremony may be assessed and some features of Himalayan Hinduism and of village Hinduism in general may be illuminated.

DESCRIPTION

The natural history of a performance of the feat in western Garhwal may be summarized as follows:[5] One or more residents of a village whose members have

1 This paper was presented in abbreviated form before the Southwestern Anthropological Association, Santa Barbara, April 1, 1961. The research upon which it is based was carried out in a village of western Garhwal during 1957-58 under a Ford Foundation fellowship. I would like to thank David G. Mandelbaum for his helpful comments on the paper.

2 Ahmed, 1891; Cockburn, 1894; Harcourt, 1871, pp. 318ff.; Moorcroft and Trebeck, 1841, pp. 17ff.; Mountaineer, 1860, pp. 41ff.; Rose, 1894b; Traill, 1828, pp. 224f. Cf. Grierson, 1916, pp. 1 ff.

3 E.g., Atkinson, 1884, pp. 834f.; Crooke, 1926, pp. 141f.; Frazer, 1913, pp. 196f.

4 Woodruff, 1947, pp. 50 ff.

5 This account is drawn primarily from descriptions by two informants who witnessed such a ceremony about thirty years ago in western Garhwal. Also helpful were the published sources cited above and an interview with Mr A. C. Chandola, a linguist at the University of California, Berkeley, who is a native of a village near Pauri, Garhwal.

been experiencing difficulties such as sickness or poor crops, or who are threatened with such difficulties, consult a shaman *(bākī)* to find out the source of the trouble. The shaman calls upon his familiar spirit which then possesses him and, in this case, announces that the troubles are being inflicted by the powerful god *Mahādēv* (Siva) to show his displeasure with villagers' inadequate worship of him.[6] Certain villages which are especially devoted to Mahadev are most likely to attribute their troubles to this deity. It is in such villages that rope sliding occurs while in others, devoted to other gods, it may never occur.

The shaman's familiar spirit may tell villagers that the rope sliding ceremony is required or, upon finding from the shaman that Mahasu is displeased, villagers may simply assume that this ceremony is required as a result of their knowledge that it has not been performed for many years. In many cases villagers do not know whether Mahadev will demand this ceremony or a lesser one until they later hold a dance in his honor where he possesses ("comes to the head of") one of the villagers (not the shaman), dances in his body, and enunciates his demands through the mouth of that person. After learning that the rope sliding rite is required, villagers vow before the god to perform it. At this time their troubles are alleviated, but they must fulfill the vow or face worse punishment. They then approach a man of the untouchable entertainer caste known as *bēda* or *baddī*, to perform the feat. The members of this caste are reputed to be devotees of Mahadev and the men who perform the feat are said to prepare for it by a lifetime of intense devotion to that god. Since the bēda caste is a small one, the performer may have to be summoned from some distance.

When a bēda man has agreed to perform the ceremony, it is publicly announced that "bēda will slide." This occurs several months before the ceremony, and the interim is devoted to preparations. The bēda and his family travel from village to village throughout the area, dancing, singing, announcing their intention to slide, and collecting donations. Meanwhile, in the area in which I worked, rope-makers were hired to make a rope 200 to 1000 yards in length depending upon where it was to be used. In most areas reported by others the bēda himself made the rope. The rope is made of a special grass *(Spodipogon augustifolius)* used in the preparation of rope bridges, and is two or three inches in diameter when completed. It is carefully tested, and soaked in oil prior to use.

6 Mahadev may also make his demands known by possessing a villager who is dancing on some occasion other than a shamanistic seance. Rope sliding is sometimes and in some places held periodically as a preventative measure without waiting until it is demanded. In the Sutlej valley it is held each twelve years at the Bihunda fair in honor of the goddess Ambika (mother of Paras Ram) and of local gods (see Harcourt, 1871; Rose, 1894b). Paras Ram is a reincarnation of Vishnu while Mahadev is identified with Siva. Sometimes the ceremony is described by informants as honoring Mahadev *and* his wife, Parbati.

A steep hill, cliff, or ravine has been selected as a site for the performance. Shortly beforehand the rope is stretched from top to bottom of the incline well clear of obstacles, and is secured to trees or posts at either end. A narrow wooden saddle, called a "horse," with a deep groove on the under side, is made by carpenters and is placed on the rope, secured by thongs or boards to the rope so that it cannot bounce off and yet can slide freely. The saddle and rope are similar in construction to one type of rope bridge used in the area.[7] The saddle is tied by a cord to the anchoring post at the upper end of the incline. The bēda must honor the god by riding the saddle down the slippery rope. At the lower end where the gradient of the rope decreases due to its own weight, a blanket or another rope is wrapped around it to increase its size and thereby brake the saddle and rider.

Near the upper end of the rope a pole is erected with a revolving horizontal cross-piece at the top. It is held in place with guy-ropes and is sufficiently stable to allow the bēda to sit or lie at one end of the cross-piece and whirl around, an activity known as *baRtakhanD* (rope ceremony, whirling).[8] I am informed that in the vicinity of Pauri, Garhwal, the pole is topped, not with a horizontal cross-piece, but with a stationary cone-shaped framework on the apex of which is a small circular platform or pivot. The bēda balances himself in a prone position on this platform and pivots on it by pushing with his hands against the framework below him. Whirling is not an integral part of the rope sliding ceremony in all areas. Where it is not, the platform may consist simply of a string cot suspended between four vertical posts.

On the appointed day people gather from many miles around to watch and derive benefit from the festive event. It is expected that the headman of each village in the area will attend to secure the benefits of the performance for all residents of his village. The bēda is bathed, dressed in white, and then carried through the assembled crowd on the back of the headman, elder or keeper of the temple of the host village. He is followed by his female relatives who dance as they go, and the bēda himself carries a tray upon which he receives donations.[9] As he goes he makes taunting, humorous, and obscene (and long-remembered) rhymes and comments about men in the crowd, who respond by giving him money to divert his attention to others. This is one of the highly ritualized occasions found widely over India in which a person of low status can publicly deride his caste superiors without fear of reprisal. The bēda may also announce peoples' names in honorific

7 See Fraser, 1820, p. 31.

8 Note the similarity to the hook-swinging appartus described by Thurston (1903).

9 In the Sutlej valley, according to Rose (1894b), funeral music is played as the bēda is carried to the rope, the five valuable items that are put in the mouth of a dying man are placed in his mouth, and his relatives mourn.

context in return for payment. He is carried to the pole or platform where he continues to perform and receive payment. There, if whirling is to be performed, he honors the headman of each village in ritual fashion by announcing the headman's name and whirling about a few times in his honor. This secures divine blessing not only to the man honored, but to all residents of the village he represents. In Pauri, Garhwal this was a ceremony separate from rope sliding. There the honoring of headmen was done in a traditional order of precedence. Any mistake on the part of the bēda in calling the names in order or in performing the feat was considered a bad omen which in former times might result in the execution of the bēda.

After the whirling performance, the rope sliding is held. A Brahmin conducts a ceremony worshipping the local gods and the god to whom the ceremony is dedicated. The bēda is blessed, rice is plastered on his forehead, and one or more goats are sacrificed to the gods to assure his success. The bēda is fed, receiving payment for each morsel of food he eats. He is then placed astride the saddle. Heavy bags of clay or sand, weighing perhaps forty pounds, are fastened to each leg, and are secured to his body with a shoulder harness. The bags are intended to help balance the bēda in his brief journey. In his hand the bēda holds a white cloth which he waves on the way down to honor the god and "to let people know he is coming." He then awaits a signal to assure him of divine protection before attempting the feat. In the case described in most detail to me, a rain shower had passed, the clouds parted and two white birds flew between them. They were assumed by the bēda to be Mahadev and his wife Parbati signalling their approval, so he gave a signal to an attendant, the cord holding the saddle was cut, and down he went at great speed, smoke trailing impressively behind as a result of friction between rope and saddle.[10]

At the bottom men stand ready to catch or assist the bēda. In this case the saddle stopped too suddenly and the bēda catapulted off but was caught by the waiting men who themselves sustained minor injuries. In a less successful earlier performance the rope was uneven and the saddle would not slide properly, so it had to be pushed much of the way. In a performance reported by Harcourt the saddle stuck, and while the rope was being tightened to facilitate the descent, it broke and the bēda fell to his death.[11] Cockburn and Traill both report that the baddī was killed by spectators if he fell in performing the feat, and this agrees with information on the ceremony as performed in Pauri, Garhwal. According

10 Cockburn (1894, p. 206) notes that "two kites must hover over his head before this is done."

11 Harcourt, 1871, p. 321.

to Moorcroft and Trebeck, however, people were stationed below to try to break his fall should he fall or the rope break.

When the bēda has successfully completed his ride he is carried to the dancing area near the village temple where some or all of the following relics of the performance are distributed among the assembled multitude: the bēda's hair, his clothes, the clay from the bags on his feet, the rice that had been plastered on his forehead, small segments of the rope, and the soil that was heaped up at the base of the whirling pole. These are taken home by those who obtain them and are placed in their fields or houses to promote health, fertility, and prosperity. The saddle is placed in the house of the man who carried the bēda prior to the performance. The bēda is given additional money, jewelry, and clothing. He is thereafter greatly honored and in western Garhwal is known by a particular title *(bēdwārth)*. According to my informants who knew an era and area where the feat was infrequently performed (every ten or fifteen years in an area of one hundred or more villages), the bēda would normally not be called upon to perform the feat again. Moorcroft, however, spoke to a man who had performed it sixteen times. The ceremony ends, as do many religious activities in this area, with a dance in honor of the village gods.

The danger in rope sliding is greatly emphasized and probably exaggerated in the folklore of the region. In many cases the rope is probably never far off the ground, and in any event the counterweights make falling highly unlikely. The main danger is that the rope might break. It is to guard against this that the bēda is generally reported to make the rope himself, just as in our culture parachute jumpers often prefer to pack their own parachutes. My informants told stories of fatal accidents, cowardly bēdas who backed out, hid, or turned into animals to avoid performing the feat. This folklore contributes tc the public appeal of this event, to the respect and remuneration received by the bēda, and to public confidence in the efficacy of the performance in pleasing the god.

ANALYSIS

One of the first questions which comes to mind about this ceremony is: Is it Hindu? Is it not alien or pre-Hindu? It does not appear in the Hindu literature. It is limited in India to the Himalayan area and it in some ways resembles a Tibetan ceremony held annually in front of the Potala at Lhasa, in which men slide down a rope at great peril to life and limb to secure benefit to the Lama.[12] This raises further questions as to the nature and definition of Hinduism. Without going too deeply into this we can say that rope sliding may not be within

12 See Bell, 1946, p. 273; Crooke, 1926, p. 142; Waddell, 1905, pp. 397f.

the great literate tradition of Hinduism, but it is, as I shall show, similar in many respects to other ceremonies widespread in India and therefore it is well within the pan-Indian non-literate Hindu tradition. The term "Hinduism" cannot be used meaningfully to exclude either tradition. In considering the affinities of rope sliding it will be useful to follow Linton's terminology distinguishing form, function, meaning, and use.[13]

At first glance rope sliding, in form at least, appears to be unique to the Himalayan hills. That is, nowhere else in India that I know of do people slide down ropes in any manner resembling that described here. In many areas the terrain would prevent it even if it were known and desired. However, if one looks at the ritual activities of rope-walkers, acrobats, singers and dancers who go by the names *bēda, baddī,* or *nāt* elsewhere in India, he will find points of similarity in form which suggest a continuous range of performances to which rope sliding might be related.[14] This suggestion is made more plausible when one considers the closely related *ganer* ceremony of Kulu reported by Rose wherein a rope is necessary and some of the ritual forms are similar, but no sliding or any imitation thereof takes place.[15] He mentions another, probably related ceremony in the Beas valley of Kulu in which men are lowered over cliffs on ropes of their own making and are honored for this feat.[16]

The famous "hook-swinging" ceremony of South India, while different in many respects, bears formal similarities to rope sliding in the suspension of the low caste performer (who is not a bēda, however) at dangerous heights from a rope, and also in the whirling around of the performer on a pivoted horizontal piece atop a pole to honor the gods.[17] The similarities between the two in function, meaning, and use are even closer than in their form. Both avert difficulty by honoring gods who control fertility, health, and prosperity.

Tibetan rope sliding, mentioned above, shows some striking similarities to the ceremony described here. However, most travel between Tibet and the lower Himalayas is by Tibetan traders and herders rather than by Indians, and present evidence would indicate that rope sliding in Tibet is limited to Lhasa while it is

13 Linton describes the qualities of a trait complex as: *form,* ". . . the sum and arrangement of its component behavior patterns; in other words that aspect of the complex whose expressions can be observed directly . . .," *meaning,* ". . . the association which any society attaches to it," *use,* ". . . an expression of its relation to things external to the social-cultural configuration," and *function,* ". . . an expression of its relation to things within the configuration" (Linton, 1936, pp. 403f.).

14 See Crooke, 1896, pp. 333f.; Russell, 1916, pp. 289ff.

15 Rose, 1894a.

16 Rose, 1894b, pp. 56f.

17 See Dubois, 1906, pp. 597ff.; Thurston, 1903.

fairly widespread in the lower Himalayas. It therefore seems more likely that the ceremony moved from India to Tibet than the reverse. Moreover, performances in the two areas are more similar in form than in function, meaning, and use.

Local and regional variations appear in the form of the rope sliding ceremony within the lower Himalayas just as they do in other aspects of culture.[18] These are typical of the distributions and variations which give regional and local variety to Hindu Indian culture. They account for many of the apparent contradictions and inconsistencies in the accounts of this and other ceremonies by different authors, and they point up the importance of specifying the level of abstraction at which one is working if culture areas based on trait distributions are to be delimited in India.

Thus, though rope sliding is superficially unique to the Himalayan hills, on closer analysis it can be seen as but one formal variant of a pattern of worship widespread in Hinduism. This similarity of rope sliding to other Hindu ceremonies becomes more obvious when phrased in terms of function, meaning, and use.

The use of the rope sliding ceremony is the same as that of a vast range of ceremonies in village Hinduism—to alleviate or avoid difficulties, especially sterility, illness, and crop failure.

The meaning of the ceremony revolves around the belief that supernatural beings are responsible for most human difficulties and that many of these difficulties are caused by deities as a means of displaying their anger or disappointment at being neglected—at not being properly worshipped. This is reflected in the widespread pattern of worship. When trouble strikes, man must first find its source, and this can most often be accomplished through the agency of a shaman whose personal spirit has direct access to information about the supernatural. He must then find what specific demands the god will make. By meeting these demands he can protect himself from the god's wrath. Generally the god demands gifts (including sacrifices), entertainment, ordeals, or mortification by the devotee.

The prevalence of dangerous performances and mortification in this context may be related to the fact that the god vents his wrath because he feels neglected. To appease him attention must be focused on him. To honor the god by publicly putting a man in pain or in danger—in the case of rope sliding by placing him literally between life and death—is to focuse attention on the god to the highest degree and so to placate the god most effectively.[19] Abbé Dubois has stated that:[20]

18 See Berreman, 1960.

19 For this point I am indebted to David Mandelbaum.

20 Dubois, 1906, p. 600.

> This tendency of Hindus to submit their bodies to severe and often cruel tortures, or to spend their means in costly offerings, is manifested whenever they find themselves in critical circumstances, and particularly in times of sickness.
>
> There is not a single Hindu who does not in such cases make a vow to perform something more or less onerous on condition that he is delivered safe and sound from his unfortunate predicament.

This "tendency" is widespread in the world. In India it is expressed in performance of the rope sliding ceremony, the whirling ceremony, hook-swinging, fire-walking, and in many cases of fasting, sacrifice, offering, or pilgrimage; in short, in almost any religious activity common in village India. All of these activities focus attention upon and honor the gods to whom they are dedicated. The sequence: trouble leading to consultation with a shaman whose information results in taking a vow and ultimately performance of the promised action to honor the god, is found almost everywhere in India.[21] In the Himalayan hills, the more serious the oversight, neglect, or misdeed by the worshippers and the more powerful the god, the more dangerous are the consequences and the more rigorous are the demands. The more expensive, spectacular, or difficult the performance or offering, the more efficacious it is. Rope sliding is a performance demanded of those who worship him by the powerful god Mahadev, and if it is not carried out the entire community will suffer severely. Other gods demand other ceremonies, but the over-all pattern is the same. These ceremonies are a major focus of the religious life of those who perform them.

There has been some speculation about the meaning of the ceremony by several of those who have described it. Frazer described it not only ". . . as a propitiation of Mahadeva . . . performed as a means of delivering a community from present or impending calamity," but also as an example of the use of a "human scapegoat."[22] There is, however, little evidence to support the latter contention, for the bēda is honored for the feat rather than being banished thereafter; relics of the performance which are most closely associated with him are sought by community members, and it is a bad omen if he should not survive. A scapegoat would hardly be treated in this fashion. If it is true, as Traill reports, that the bēda becomes sterile as a result of the performance, one might assume that he had taken onto himself the sterility plaguing community members. There are, however, no other reports of the bēda assuming the difficulties of the community he serves nor is there any associated ritual activity which would suggest such a transfer.

Rose[23] and Cockburn assert that rope sliding is a form or derivative of human

21 Cf. Harper, 1957; Opler, 1958, p. 554.

22 Frazer, 1913, pp. 196f.

23 Rose, 1894b.

sacrifice. If so, it would not be unique in India. Human sacrifice is described in the *Kali Purana* and is widely reported for earlier times in both tribal and Hindu India, especially in the Himalayan area.[24] Rope sliding as practiced in Garhwal, however, shows no evidence of sacrificial intent. On the contrary, every precaution is taken to protect the bēda, and he is rarely killed in the performance. In view of this, the preparations for the bēda's death which precede the ceremony in the Sutlej valley as reported by Rose might be interpreted as insurance against the unhappy possibility of the bēda's death, to assure him a satisfactory place in the hereafter, and to protect those who have sent him on his dangerous journey from his potentially angry spirit, rather than as evidence of intent to sacrifice.[25]

Both Cockburn and Traill report that in former times the bēda was killed by bystanders if, despite precautions, he fell. This was also true in both the whirling ceremony and the rope sliding ceremony in Pauri, Garhwal. It is not indicative of sacrificial intent, however. The performance was held to honor the god by a performer who was assumed to be a devotee of that god. If it was done properly, the god was pleased, trouble was averted, and the performer was honored. If the performer fell or otherwise failed to properly perform the ceremony, it was considered to be a bad omen; it offended the god or was evidence that the god was offended by the performer. Moreover, the performer was thereby proved to be an inauspicious person who would bring further difficulty to the community. He was therefore dispatched to protect the village from further divine retribution. He had failed to appease the god, offending him instead, and he was best removed from the scene. His heirs, in such a case, received no payment. The performance was considered to have been a failure.

The important point is that in no place was the death of the bēda sought or considered other than inauspicious. It was everyone's hope and intent that he should succeed, for only then would the god be satisfied and the village prosper. It was a bad omen if the bēda failed; there was rejoicing if he succeeded. In a sacrifice the reverse is true: it is a bad omen if the victim survives even briefly; a good omen if it is sacrificed expeditiously. There is therefore no evidence that this is a sacrificial ceremony. If in the Sutlej area it has acquired some of the trappings of sacrifice, it has probably done so because it occasionally resulted in death rather than because death was the intended result. The primary meaning of the ceremony seems always to have been the propitiation of a deity by the performance of a spectacular, expensive, and dangerous feat in his honor which had the effect of focusing attention upon him to a maximal degree.

24 Cf. Basham, 1954, pp. 336f.; Cockburn, 1894; Dubois, 1906, pp. 645ff.; Francke, 1914, pp. 4, 12f.

25 There is great fear, at least in this area, of the restless spirit of a person who has died unprepared or improperly mourned.

The functions of the rope sliding rite are numerous but, like its meaning and use, are common to many Hindu ceremonies. Above all, it relieves anxiety by reassuring people in the face of impending calamity. It is likely to be performed when, for example, a cholera epidemic threatens an area in which the god Mahadev is worshipped prominently. At the same time a rash of other ceremonies and sacrifices occur, all aimed at appeasing the gods and thereby protecting the village. The bēda with whom Moorcroft talked was credited with having saved his part of Garhwal from cholera when he performed the feat during an epidemic. In the village in which I worked, where Mahadev was not the most important god, a low caste woman saved the village from a cholera epidemic by becoming possessed of the goddess who was causing the illness there, and directing villagers in the correct propitiatory worship.[26] The rope sliding ceremony therefore functions as does much ritual in village India and reflects the "prevailing health anxiety" described by Opler.[27] It does this on a village or regional scale rather than, as is more commonly the case, on a family, lineage, or local caste level, but there are other ceremonies of community scope in Hinduism such as the village protection rite performed in this hill area.[28]

A distinction can be made in the village of my research between "household gods" and "village gods." The former are worshipped on a household (joint family or sometimes lineage) basis and their effects are felt by household groups. The village gods, such as Mahadev, affect the entire community and are usually worshipped on a village-wide basis.[29]

Like the village protection rite and various agricultural ceremonies which honor village gods, rope sliding is sponsored by the village at large. All are expected to participate and all who participate reap the benefits. Participation and consequent benefit actually extend far beyond the village, though the sponsoring village is always more deeply involved than others. Since it requires the participation of several castes and benefits all of them, it has an integrative function, emphasizing the interdependence of villagers and their common ties to the locality. A shaman, who in this area is usually of an untouchable caste, identifies the trouble and often is instrumental in determining remedial action.[30] A Brahmin carries out worship of

26 Berreman, 1959, p. 192.

27 Opler, 1958.

28 Berreman, 1959, pp. 183ff., 206ff., 219.

29 *Idem*, pp. 168ff.

30 Shamans may be of any caste but in this area the great majority of them, as of most supernatural practitioners, are of low caste. In the lower Himalayas there are no Sudra castes. All castes whose counterparts on the plains of India would be Sudras are here untouchable *dōms* and it is from these castes that most shamans come. Cf. Berreman, 1960, pp. 775f.

the god to be honored and of other village gods. Music is provided by village musicians, an untouchable caste. The saddle is made by carpenters; the rope may be made by a special rope-maker caste; and images symbolic of the gods being worshipped are made by the blacksmith. The untouchable bēda performs the feat itself. All of these people are in the employ of high caste agriculturalists who contribute most heavily to the support of the ceremony. All of these groups depend upon one another, albeit the distribution of power among them is less than equitable.

At this time factional splits, traditional animosities along kin and caste lines, are suppressed and all are expected to coöperate for the common good. This drawing together comes when it is most needed, at a time of crisis, for it is at times of crisis that rope sliding is performed. The need to perform a community ceremony may lead to enforced resolution of disruptive conflicts in order that all may participate. The death of a prominent and highly respected woman in the village I studied had such a result when factional differences threatened to prevent the proper celebration of her funeral. Of course, more serious conflicts may actually prevent the performance, or the proper performance, of such a ceremony. Cases of this kind are also on record, but they are unusual.[31]

Rope sliding, like most other village religious activities, and like fairs has an important social function. People from a wide area converge on one place, exchange news, and find amusement in one another's company. It affords enjoyable entertainment and a welcome diversion, on a grand scale, from normal routine just as a sacrifice, seance, or dance does on a smaller scale.

As with many ceremonies, rope sliding provides a means to acquire prestige among those who support it. Religious ceremonies are one of the relatively few approved outlets for conspicuous expenditure of wealth in this area and rope sliding is one of the most expensive of these. There is an opportunity for all to acquire prestige because support is solicited from the entire community and from all residents of other villages who can be induced to attend. High caste agriculturalists are the primary ones to derive prestige in this manner because they are in the best position to contribute money or goods.

The ceremony also provides an arena in which individual low caste participants, especially bēdas and shamans, can acquire prestige and can exert initiative and influence that is otherwise denied them; influence that is far out of proportion to their low social and ritual status and their dependent economic condition. In a sense this may be an integrative function too in so far as it serves as a balance and possible "safety valve" for the expression of suppressed feelings and capabili-

31 See Berreman, 1959, pp. 211, 534f.

ties and the realization of otherwise inaccessible aspirations by depressed groups in this rigidly hierarchical and inequalitarian society.

This is a feature of Indian village religious ceremony which is often underemphasized. In the part of the lower Himalayas with which I am most familiar, I would venture to say that Brahmins play a smaller and much less creative role in village religion than do various, usually low caste, non-Brahmanical practitioners such as shamans, diviners, exorcists, and spell-casters. Low caste shamans diagnose difficulties and often recommend corrective action. Low caste practitioners direct most of the activities calculated to appease the deities or exorcise the spirits. They cast spells, counteract spells, detect theft, find lost objects, cure illness, foretell the future, read omens, and do a host of related activities. It is they who determine what gods are worshipped, in what manner, and under what circumstances. They introduce new gods and forms of worship and occasionally eliminate old ones. They often determine or influence who in the community has to perform a ceremony, when it must be performed, and how expensive it will be. In this role they not infrequently pass judgment on disputes. They hold the welfare of particular households or entire villages in their hands. They do this not as low caste people, but as technical experts or as vehicles for the voices of the supernatural. They are not unaware of their role, however, as they point out their indispensibility to the community with evident satisfaction. The most effective limitation to their power over their clients is that they are numerous and specialized. Their clients do not hesitate to consult others of their profession if the advice of one seems inaccurate or unreasonable, and no one practitioner is competent in all types of cases. Brahmins, by contrast, are important mainly in life-cycle ceremonies and in performing formal worship. The relatively minor role of the Brahmin in the rope sliding ceremony is typical of his role in the ceremonial life of this culture area. It would be worth while to investigate the religious roles of Brahmins and non-Brahmin practitioners elsewhere in India with regard to their spheres of influence and their potential for innovation in religious behavior and belief.

COMMENTS ON CHANGE

The decline in prevalence of rope sliding is not adequately explained by describing the roles of these religious practitioners. At one time rope sliding was held frequently enough that Moorcroft was able to interview a man in about 1820, who had performed it sixteen times and Traill commented in 1828 that "no fatal accident has occurred from the performance of this ceremony since 1815, though it is probably celebrated at not less than fifty villages each year."[32] By the latter

32 Traill, 1828, p. 224.

half of the nineteenth century, the practice was said to be outlawed in British territory, persisting only in the princely state of Tehri Garhwal and perhaps in other independent territories.[33] At the Nirmand fair in the Sutlej valley in 1868, and thereafter, a goat was substituted for the bēda as a result of the ceremony's having been outlawed following a fatal accident,[34] and for many years in the vicinity of Pauri, Garhwal, an image has replaced the bēda as a result of British regulations. However, British sanctions have not been the only factors which have led to decline in the ceremony. Economy may have played a part. In the area of my work a milder ceremony (*baʀtatōʀ:* rope sliding, breaking) was often performed under circumstances similar to those which inspired rope sliding. In this version a rope was stretched and a wooden frame representing a man was moved along it. Later a bēda rubbed his hand over it, after which it was broken in a tug-o-war, whereupon Mahadev was thought to be satisfied. This ceremony was doubtless used long before the British outlawed the sliding feat, evidently as a cheaper alternative. The bēda got two rupees and some grain, an infinitesimal cost when compared to the cost of rope sliding.

A third factor in the disappearance of rope sliding is what might be called "plainsward mobility." That is, the trend toward adoption of the behaviors of plains dwellers. The goal of many hill people is to be acceptable or respected in the eyes of plains people with whom they are increasingly in contact and who are usually in positions of relative advantage as officials, merchants, tourists, keepers of temples, etc. This involves adoption of behaviors which, in the hill man's view, will counteract the prevalent "hill-billy" stereotype of the hill man. This means, besides adopting plains customs, giving up (at least before outsiders) things that are reprehensible by plains standards and also things which are merely distinctive of hill people. Prominent among these are buffalo sacrifice, bride-price, polyandry, and other conspicuous features subject to ridicule by plains people, including the rope sliding ceremony.

The extent to which this process occurs depends, of course, on the amount and kind of contact between hill and plains people in any given area. This is not an unusual process in India. It is of the same order as "Sanskritization," described by Srinivas, which has been involved in the mobility of castes from time immemorial.[35] It resembles Westernization and urbanization, both of which have been prominent in India for many years. Plainsward mobility combines elements

33 In the area in which I worked it was last performed about thirty years ago. Very likely it has been performed more recently in other, perhaps more remote, areas. It is performed to the present day in the vicinity of Pauri, Garhwal, with an image substituted for a man in the saddle.

34 Harcourt, 1871, p. 321; Rose, 1894b.

35 Srinivas, 1952, pp. 212ff.

of each of these processes to the extent that the plains people whom hill people attempt to emulate are Sanskritized, Westernized, and urbanized. Its result is, among other things, a decreased demand for the rope sliding ceremony. Perhaps another result is a decrease in the importance of the shaman relative to the Brahmin in village religious life as Sanskritic values take hold.

At the same time the performers themselves, bēdas, are both upwardly and plainswardly mobile. Consequently they are reluctant to play their traditional role in the ceremony, a role which identifies them as hill people of very low status. Recently, for precisely these reasons, the bēdas in the area of my research had given up most of their entertainment and ritual functions, and they planned to give them up entirely in order to become farmers.[36]

In the processes of change to which it has been subjected, rope sliding therefore follows a pattern of change in ceremonial usage, under conditions of outside contact and social mobility, common to much of India.

CONCLUSIONS

The Himalayan rope sliding ceremony, while spectacular, need not be viewed as a unique and inexplicable performance. It is a Hindu ceremony well within the range of ceremonies found in villages throughout India. That is, it is equivalent in function, meaning and use to many ceremonies of propitiation of deities in India. In form it is distinctive but even in this respect it is not without affinities elsewhere in India, for similar and probably related forms can be cited. It might best be described as a sub-regional expression of a pan-Indian tradition.[37] That such a pan-Indian tradition exists outside of the literate tradition emphasizes the importance of recognizing the dimension of "spread" of traditions[38] within civilizations as well as the "great" and "little" traditions.[39] Hinduism and Indian civilization are characterized as much by their non-literate traditions as by their literate traditions. Some of the former are as widespread as the latter, and are doubtless known and practiced by far more people than many aspects of the literate tradition. Little and local traditions must not be confused, for "little" traditions are often of all-India spread.

Rope sliding exemplifies the necessity of viewing Hinduism in the totality of its expressions; in meaning, function, and use as well as form, and in viewing each aspect in cultural context. When this is done, Himalayan Hinduism, as exemplified by Pahari-speaking Hindus, will be seen as a regional variant of the all-Indian

36 See Berreman, 1959, p. 119.
37 Cf. Cohn and Marriott, 1958, p. 2.
38 Srinivas, 1952, pp. 213ff.
39 Redfield, 1956, pp. 70ff.

and North Indian Hindu traditions rather than, as has often been asserted, a peculiarly deviant, degraded, or attenuated form of Hinduism or even a non-Hindu "tribal" religion. Pahari-speakers are not more tribal by religion than by language, economy, or any other aspect of their culture. By almost any criterion that may be applied they are a distinctive subgroup within the North Indian "genetic unit"[40] and the North Indian culture area.

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ASYMMETRIC MARRIAGE RULES, STATUS DIFFERENCE, AND DIRECT RECIPROCITY: COMMENTS ON AN ALLEGED FALLACY

EDMUND LEACH

THIS PAPER refers to Barbara Lane's article "Structural Contrasts between Symmetric and Asymmetric Marriage Systems: a Fallacy,"[1] the argument of which is itself by no means free from fallacy.

In her first paragraph Dr Lane states firmly that she is discussing "prescriptive" marriage systems. This use of the word "prescriptive" is a technical term introduced into English language discussions of the topic by Rodney Needham. It refers to the fact that in certain societies all legitimate marriages are between a man and a woman who stand in a particular kin relationship. Thus, if a male ego classifies the unmarried females of his society into several terminological categories a,b,c, etc., it is characteristic of a "prescriptive" marriage system that marriage with a member of one and only one of these categories is strictly legitimate.

Discussions of prescriptive marriage rules, in this sense, are quite distinct from arguments about marriage choice or marriage "preference." It has been reported of a great variety of societies from all parts of the world that there is a "preference" for marriage with the "mother's brother's daughter," or the "father's sister's daughter," or the "father's brother's daughter," or some other specified near relative. The great majority of these societies from which marriage preferences have been reported do not have prescriptive marriage rules; where prescriptive marriage rules exist they do not necessarily imply marriage preferences, and where prescription and preference both occur they need not coincide.

This distinction may usefully be illustrated by the classic example of the Trobriand Islanders for, in this society, there is a sort of prescriptive rule which does not entail a prescriptive system and there is a sort of marriage preference which does not coincide with the prescription.

A Trobriand male distinguishes among his kinsfolk several categories of unmarried female. Of these there is one and only one—the category *tabu*—which constitutes a set of marriageable women. This category *tabu* embraces a large number of individuals among whom are a male's true father's sister, his true father's sister's daughter, and his true daughter's daughter. Ego is ordinarily expected to marry a girl from the local neighborhood and the majority of such girls will be classified as kinsfolk of one sort or another. As far as such relatives are concerned

1 Southwestern Journal of Anthropology, vol. 17, pp. 49-55, 1961.

the rule is prescriptive, a man must marry his *tabu* and no other relative. However, the Trobrianders cannot be properly described as having a "prescriptive marriage system" in Needham's sense since there are some females within the Trobriand ego's social word who are not rated as kinsfolk at all but as "outsiders" or "strangers" *(tomakava)*, and it is legitimate for ego to marry a *tomakava* female. As between marriage with a *tomakava* and marriage with a *tabu*, marriage with *tabu* is preferred. Malinowski went farther than that and alleged that there was a preference for marriage with the true father's sister's daughter. Later investigations by Powell[1a] have showed that while the statistical incidence of marriage with *tabu* is high, the incidence of marriage with female relatives of other categories is also considerable, and marriage with the true father's sister's daughter is very rare. Malinowski's statement thus applies to a verbalized preference rather than to an actual preference.

That some of the relevant Trobriand facts remain obscure may readily be conceded, but that it is desirable in such a case to maintain a very clear distinction between the notions of marriage prescription and marriage preference seems self evident.

Dr Lane is, I feel sure, well aware of this critical distinction between the technical meaning of the terms *prescription* and *preference* and it is therefore unfortunate that at p. 55, where she comes to formulate what looks like a generalized hypothesis, she should suddenly switch from a discussion of prescription to a discussion of preference. She claims that "ethnographic reports indicate a high correlation of the matrilateral preference in patrilineal societies whereas the patrilateral preference is usually recorded in societies following matrilineal descent." Just what matrilateral and patrilateral preference mean in the context is far from clear. If for example "patrilateral preference" means "preferred marriage with the true father's sister's daughter" the proposition may well be true but it has not obvious bearing upon the issue of prescription which has been discussed up to this point. If on the other hand the word "preference" is a slip for which we ought to read "prescription," then we must again ask Dr Lane to explain what "patrilateral prescription" means and to demonstrate that such an institution could possibly exist.

Dr Lane herself notes at p. 53 that Needham has challenged the possibility of a true system of "prescriptive patrilateral cross-cousin marriage," but she insists

1a Powell, 1956, p. 314. "Of 85 marriages, one was with F sis d, one with M b d; most of the rest were between persons who by some reckoning or another could have been counted before marriage as *tabu* to each other . . . but 53 of the marriages were the only extant affinal link between the two sub-clans concerned."

that despite the theoretical objections "we may [still] consider prescriptive patrilateral cross-cousin marriage as a hypothetical case." Dr Lane, it would appear, must be unaware of Bertling and Philipsen's statistical analysis of forty-two societies which have been reported as having a rule of unilateral cross-cousin marriage. The outcome of the analysis is as follows:

	Marriage with		
	M b d	*F sis d*	*Total*
Marriage preference	20	5	25
Prescriptive marriage rule	14	0	14
Ethnographic details inadequate			3
			42

Bertling and Philipsen[2] assert flatly that "the marriage with the F sis d does not occur at all if the unilateral cross-cousin marriage is prescriptive."

It is of course possible that in making assertions about "patrilateral preference" in this context Dr Lane does not mean to refer to either preferred or prescribed cross-cousin marriage as ordinarily understood but to a mixture of the two. As indicated above, the Trobrianders have a marriage prescription of a sort but, even if we ignore the possibility of marrying strangers, the prescription that a man should marry his *tabu* allows him to marry a wide variety of females only some of whom are related to him through his father. If this is the sort of thing that Dr Lane is writing about she should make her meaning clear. It is not what most readers will think that she meant to say.

All this by way of introduction to show that Dr Lane's seemingly straightforward thesis rests in terminological ambiguity. Let us proceed. From ambiguity to error is but half a step.

The "fallacy" with which Dr Lane's paper is supposed to be concerned is specified at the top of page 50. She claims that unnamed anthropologists have argued that, in a system of prescriptive matrilateral cross-cousin marriage of the type which the Dutch used to call "circulating connubium," "because direct reciprocity in kind is never practiced, every group standing in the relation of wife-giver to some second group and wife-receiver to a third group, this sort of system implies status inequalities between the groups involved. . . ."

She then goes on to demonstrate that this is a "fallacy." In particular she shows that if four patrilateral lineages operate a system of circulating connubium

2 Bertling and Philipsen, 1960, p. 72.

of the type specified, such that group A gives wives to group B, which gives wives to Group C, which gives wives to group D, which gives wives to group A, then there exists a direct reciprocity between groups A and C and between groups B and D in that the sister's daughter of a man of the A group can be thought of as being directly exchanged for the sister's daughter of a man of the C group and that similarly the men of groups B and D can be represented as exchanging their sister's daughters on a reciprocal basis. The logical fallacies that have found their way into this argument are quite remarkable.

In the first place as to "status inequality." If two lineages intermarry on an exclusive asymmetrical basis whereby there is a relationship of perpetual alliance, the A's giving wives to the B's but the B's not giving wives to the A's, then clearly, by definition, "an inequality of status" *with respect to the institution of marriage* exists. In most, perhaps all, societies where such an asymmetric prescription exists the difference in status between "wife givers" and "wife receivers" is clearly formulated in the spoken language—thus Kachin *mayu/dama,* Lakher *patong/ngazua,* Batak *hulahula/anak boru,* Gilyak *axmalk/imgi.*[3]

This inequality with respect to the giving and receiving of women may, or may not, be reflected in other forms of inequality, e.g., inequality of economic and political status. The following very general set of statements regarding marriage systems of this kind have emerged from the writings of Lévi-Strauss, Needham, myself, and other recent writers on this topic:[4]

(a) Where the marriage chain is genuinely circular, in the empirical as distinct from the "model" situation, then the marriage rule does not imply any absolute difference of economic, political, or ritual status between the intermarrying groups.

(b) Empirical, as distinct from "model," systems of circulating connubium are complex rather than simple; that is to say, whereas the model represents the total society as consisting of a limited number of exclusive groups all of which intermarry in such a way as to form a single perpetual marriage cycle, the empirical data relate to a large number of smaller groups linked dyadically in unstable pairs as wife-givers and wife-receivers. Sometimes the chains of alliance formed by such dyadic links work round into a circle, sometimes they do not. Among the Kachin such circular chains *(hkau wang hku)* are recognized by the people concerned.[5] Needham has demon-

3 Cf. Needham, 1961, p. 111.
4 Cf. *idem,* pp. 116-117 for bibliography.
5 Leach, 1951.

strated that comparable circular chains occur among the Purum, perhaps unrecognized.[6]

(c) Where chains of alliance formed by a succession of dyadic ties between groups linked perpetually (or on a long term basis) as wife-givers and wife-receivers do not work round into a circle, then there is an *a priori* probability that the links in the chain correspond to a hierarchy of absolute status of an economic, ritual, or political kind. It has been established as a fact that in a number of empirical cases this is so; in other words, dyadic links of the wife-giver/wife-receiver kind can be used to express political, economic, and ritual inequality.

(d) It has been recognized since the very beginning of this discussion that the ethnography includes accounts of societies with open-ended, non-circular, alliance chains of this kind in which absolute political, economic, and ritual inequality is denied. The type example of this is provided by Lloyd Warner's account of the Murngin. In an earlier article[7] I devoted considerable attention to this case, showing how the Murngin, despite the open-endedness of their system, were able to represent this system to themselves as one of reciprocal exchange between moiety groups. Later Nakane[8] showed how the rule of the matrilineal Garo, which requires that the hamlet head should be succeeded by a sister's son who is married to his own daughter, is similarly a marriage prescription of the type under discussion but that this is perceived by the Garo as a reciprocal exchange of men between paired clusters of matrilineages.

Dr Lane's proposition that systems of "prescriptive matrilateral cross-cousin marriage" operating in a circle do not necessarily involve "status inequalities between the parties" is thus peculiarly inept. If she means by this that a pair of lineages standing in a dyadic wife-giving/wife-receiving alliance are of the same status in respect to the institution of marriage, then plainly her assertion is self-contradictory; if, however, she means that such lineages need not be of differing status in absolute terms (economic, ritual, political) then her battle is vain, since no one, so far as I am aware, has ever suggested that such inequality of status was either necessary or even probable.

But now as to the Pentecost case which Dr Lane presents as a model structure of four patrilineages "marrying in a circle," each man exchanging his sister's

6 Needham, 1958a, 1961.
7 Leach, 1951. 8 Nakane, 1958.

daughter with the sister's daughter of a partner who is a member, not of the adjacent lineage but of the next but one; thus in a series A,B,C,D, A and C exchange nieces, and B and D exchanges nieces. Granted that this might possibly happen, one cannot help wondering why Dr Lane describes this as a "patrilineal" system, for certainly it is a very odd kind of patriliny. The model requires, among other things, that, for purposes of marriage, a girl is under the jural control of a male of the status of mother's brother while her full brother is under the jural control of his own father. Put in another way it requires that a girl is not thought of as a member of her own birth lineage at all but as a chattel belonging to a patrigroup composed exclusively of males and comprising the girl's mother's father, mother's brother, mother's brother's son, and so on. Furthermore, despite the fact that, in this society, the "patrilineages" are truncated groups consisting exclusively of males, property rights in women exist at one remove; the males of a patrilineage have no rights over the marital destiny of their own daughters, yet they exercise control over the daughters born to these daughters.

It is always rash to assert of an assemblage of facts recorded in ethnography that "this is plainly impossible," but Dr Lane's model seems scarcely plausible. Her system cannot be as she asserts (p. 50) "a direct reciprocal exchange of women" between male groups.

Her argument would have been more acceptable and interesting if she had said that her model represents a system of normal type patrilineages linked in wife-giving/wife-receiving alliance such that any dyadically linked pair of such lineages feel themselves to be a unity which exchanges women on a reciprocal basis with one or more other such units.

The merit of this formulation would be this: (i) it does not require us to accept the peculiar notion of a group of patrilineally associated males who control the marriage of their daughters' daughters without having any control over the daughters themselves; (ii) it does not simply restate as a special case what has already been said in a more general form with respect to the Murngin and the Garo; (iii) it draws attention to a feature of open-ended hierarchical forms of this kind of system which has not previously received adequate attention.

Let me elaborate this last point. Among both the Kachin and the Lakher "matrilateral cross-cousin marriage" is used by aristocrats to express political superiority. In such a hierarchy, a chief may be father-in-law to a village headman, a village headman father-in-law to an ordinary villager.[9]

In both societies such aristocratic marriages are associated with large and highly complex bride-price payments. A feature of these payments is that gifts are made

9 Leach, 1951.

not only to males of the patrilineage of the bride's father but also to males of the patrilineage of the bride's mother's brother. Among the Kachin these latter payments to the bride's mother's brother's group are smaller than the payments to the bride's own group but, in the Lakher case, the two payments are nearly identical.[10] Dr Lane's material suggests that a useful way of describing the Lakher situation would be to say that a pair of dyadical linked lineages *(patong/ngazua)* act as a unity with respect to the marriages of the daughters of the *ngazua*, that is the junior member of the couple, and it is on this account that the total of the bride price is divided equally between the two groups. In Dr Lane's special four lineage cycle there is, presumably, no bride-price, but, if a pair of dyadically linked lineages (e.g. A + B) act as a unity with respect to the marriage of the daughters of B—the wife-receiving member of the dyad, then the total group A + B would be antithetical to a similar group C + D and the marriage of the daughters of D to the males of A might quite reasonably be thought of as a reciprocal exchange for the marriage of the daughters of B to the males of C.

Such a formulation of the facts would also suggest analogies with the Trobriand case where, as an adjunct to an asymmetrical marriage rule, the marital destiny of a girl of matrilineage A, whose father is of matrilineage B, is evenly shared between the B father and the A mother's brother.

But Dr Lane will not have it this way. It is not clear whether, at p. 52, she is discussing a model or reporting empirical ethnographic facts; but, if the latter is intended, then something has been omitted from her account. The issue of jural authority which she dismisses as irrelevant is in fact crucial. How could it be possible that a man should have authority over his daughter's daughter when he has none over the daughter herself. Surely it is more probable that authority over girls is shared between their "fathers" and their "mother's brothers." If this is so, then Dr Lane's statement that this is a direct reciprocal exchange of women between men of groups A and C and between men of groups B and D is fallacious. The exchange must be between groups A + B and C + D and between groups B + C and D + A.

I have already explained why the tail end of Dr Lane's paper, which mixes up the notions of marriage prescription and marriage preference, does not merit serious examination. Nevertheless I would make one further comment. As her title indicates, one of the principal points which Dr Lane seeks to establish is that the category distinction between "symmetric" and "asymmetric" systems of marriage regulation is a good deal less fundamental than has sometimes been argued. It is of interest that Lévi-Strauss, who at one time laid great stress on this dis-

10 Parry, 1932; Leach, 1957.

tinction, has reached a similar conclusion.[11] Lévi-Strauss' argument is, however, precisely the opposite to that advanced by Dr Lane. Dr Lane tries to show that, in empirical cases, an asymmetric system (which requires a minimum of three distinct groups if it is to operate at all) may become equivalent to a symmetric system in which there is a direct exchange of women between isolate groups. Lévi-Strauss, on the other hand, argues that, from a mathematical point of view, a direct reciprocity between two groups A and B is simply the limiting case of a circular exchange A→B→B′→A in which B and B′ coincide. He therefore argues that the general theory should concern itself with circulating systems, treating dyadic reciprocity as a special case of circularity. I must confess that if I had to choose between these two points of view I should find that of Lévi-Strauss by far the more illuminating. I fear that, somewhere along the line between Dr Lane's informants' statements and her diagrams, facts and models have become inextricably confused but I would commend to her attention a sentence from Needham's latest paper on this topic: "It is through the dyadic mode of relation in terms of which social life is framed that a dualistic symbolic classification may be related directly to a system of asymmetric alliance."[12]

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FIELD TESTING OF CULTURAL LAW: A REPLY TO MORRIS OPLER

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THE SPRING 1961 issue of the *Southwestern Journal of Anthropology* carried an article by Morris E. Opler entitled "Cultural Evolution, Southern Athapaskans, and Chronology in Theory." Since this is a critique of an article of mine[1] that may be inaccessible to many readers of this journal, it may assist them in evaluating the comment to know what the original article said on the points in dispute.

1. Opler quotes my statement that the law of energy and cultural evolution was first made explicit in 1943, then asks, "Can Dr. Meggers be serious in telling us that this point of view was first set forth in 1943 . . .?"[2] A law and a point of view are not the same thing. The idea that energy or technology exerts a more controlling effect than other aspects of culture is an old one. Expressing the idea as a formula subject to experimental testing is what was new, and this was done explicitly and at length by Leslie White in 1943.[3] The formulation has been referred to by others as "White's Law"[4] without provoking Opler's displeasure. The 1943 date is also important because the appearance of White's article in the *American Anthropologist* made it impossible for any serious anthropologist to remain unaware of the law or its implications.

2. Opler claims that I take credit for a "new idea" in arguing for experimental testing of the law.[5] He quotes Marxist theorists at length to show that they applied it years ago. This is not news to me, nor was I trying to conceal their interest in the subject. I simply was not writing an historical article and consequently did not consider the information relevant. My introductory sentence confines the discussion to American anthropology, and I expressed regret that American anthropologists had not attempted to subject the law to field testing. I accounted for this lack of interest by the generally current assumption that the law was inapplicable to specific cultures or local sequences, and tried to demonstrate with several examples that this assumption was unwarranted.

3. Opler attacks me as an authority on the Athapaskans in a series of paragraphs beginning, "Is it true, as Dr. Meggers asserts (implies, would have us be-

1 Meggers, 1960.
2 Opler, 1961, p. 8.
3 White, 1943.
4 Steward, 1953, p. 318.
5 Opler, 1961, pp. 8, 12, 13.

lieve, etc.) . . ."[6] He neglects to mention that all of the information he attributes to me is presented in my article in the form of lengthy quotations from the original author, Charles R. Kaut.[7] If Opler wishes to dispute Kaut's data, it would seem more appropriate to do so directly with Kaut. If I have chosen a poor example (which I am not convinced of), it is unfortunate, but I was aware of this possibility because none of the studies cited was undertaken to show what I wished to illustrate. Having no first hand knowledge by which to evaluate them, I prefaced this portion of my article with the following words of caution: "It should be noted . . . that good case studies are difficult to find because little attention has been paid to the kind of analysis advocated here. . . . Attention should be focused on the kind of analysis being made rather than on the validity of the interpretation in regard to the particular culture used as an example."[8] I had hoped that this would forestall attacks like Opler's, which distract attention from the main theme.

4. Opler accuses me of seeing revolutionary changes in Athapaskan social organization resulting from minor subsistence differences, and demonstrates that none exist.[9] My actual conclusion was that the situation described by Kaut "throws into relief a small segment of the process by which a minor alteration in the energy resource produces a minor change in the social organization, although the latter continues to be kinship based."[10] Opler objects to calling such small changes evolutionary, but evolution is an accumulation of small changes and it seems reasonable to suppose that if we can isolate and understand them we will be better able to understand the operation on a larger scale.

I undertook the article to which Opler objects because it seemed to me that too much attention is paid to talking about theory, defining terms, and tracing the history of ideas, and too little is devoted to seeing if or how theories work. Opler's dismissal of my efforts is discouraging confirmation of the existence of the state of affairs described in my final paragraph:

> In physics, when an experiment seems to invalidate a law, the experiment is repeated and carefully analyzed for flaws before its results are accepted. In anthropology, however, any situation that seems superficially to be irreconcilable is seized upon to demolish the law without further ado. There has been little serious attempt to analyze the data in the manner of a scientific experiment, to discover factors that might explain seeming contradictions, or to determine whether the cause of the apparent lack of conformation to the expected results might not stem from a faulty analysis of the situa-

6 *Idem*, pp. 2-6.
7 Kaut, 1957.
8 Meggers, 1960, p. 308.
9 Opler, 1961, p. 7.
10 Meggers, 1960, p. 313.

tion under examination. Until we learn to subject cultural theories to . . . "experimental" investigation . . ., we will have no true way to measure their validity.[11]

For the benefit of Opler and others who may share his views, I wish to record that I do not claim this to be a new idea, or one that I originated; I simply believe that it is the best approach at our disposal to the solution of some of the problems at hand.

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11 *Idem*, p. 315.

THE PROCESS OF FOSSILIZATION*

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FOSSILIZATION is a general term, which is frequently interpreted according to the user's interests. Some investigators have attempted to make the word very specific, while others have included a number of concepts under this one term. The actual causes or processes of fossilization *per se* tend to become obscured by the many attributes and meanings which the word has acquired. This paper is concerned not with proposing a new definition of the term fossilization, but rather with setting forth the processes involved; when do they begin and how do they occur, that is to say, the dynamic aspects of fossilization.

In his book, *Adam's Ancestors* (1953 edition, page 50) Prof L. S. B. Leakey stated his concept of fossilization, that is the specific or delineated definition of the word.

> The terms "mineralization" and "fossilization" are often employed interchangeably. Mineralization, strictly speaking, implies either impregnation with minerals or complete replacement, fossilization denotes that at least a part of the animal fats and gelatine have been lost, but not necessarily that they have been replaced by anything else.
>
> The state of preservation—mineralization or fossilization—of a bone, whether of a human or of other animal, does not alone give any real indication of its age. Frequently, however, a great age is attributed to a specimen because it is heavily mineralized or fossilized, and equally often a high antiquity is denied to some specimen because it is not heavily mineralized or fossilized.
>
> Neither of these assumptions is justifiable on the basis of the present state of our knowledge.
>
> The only use to which the state of preservation of a bone can be properly applied, is a comparative one for use as contributory evidence.

In Leakey's statement a number of different ideas are contained, which in one form or another have become thoroughly incorporated in the word *fossil* as it is generally conceived. As a group these ideas tend to cluster around two basic concepts. One is that of *age*. Age implies that the object denoted a "fossil" persists as such, usually buried in the earth for a shorter or longer time, the duration of which may be of great significance, and also there must be involved the aspect of a beginning and an ending. The other is of *condition*—the state of preservation in which the object is found at the moment of discovery. Antecedent to condition,

* Acknowledgment is hereby made with thanks for the time made available to one of the authors by the Miller Institute of Basic Research in Science.

at the end of the life span of the object, is the beginning of fossilization which is, of course, *change*, and change is the essence of the process of fossilization. Underlying the concepts of both age and condition is the cardinal prerequisite that the object be still recognizable as the physical form of a plant or animal, or one of its parts. At the same time there is no necessary direct association between the antiquity of the fossil and its physical or chemical condition. This point is emphasized by Leakey, and is conceded by both palaeontologists and archaeologists or any other investigators who have attempted to make an accurate dating of a human or animal fossil either by visible appearance or laboratory analysis of constituent structures or substances. An excellent formulation of this principle is that enunciated by Dallemagne, Baud and Morgenthaler (1956) in their study of calcium exchange in fossil bone (translation ours):

> The most striking conclusion from all these studies is that the various characteristics observed bear no relation to the age of the fossils, and that the nature of the terrain in which they were found and the physical or technological influence to which they were subjected are those factors which alone could modify their properties.

Although determination of age or antiquity, i.e. the duration of the time span of a fossil, by intrinsic examination has hitherto been unsuccessful, it is common knowledge that extrinsic criteria, such as are provided by stratigraphic or cultural association, have been instrumental in dating numerous fossils of both man and animals. Furthermore the new radiological techniques promise before long to bridge the chronological gap between the fossil itself and its physical and cultural environment.

A second problem of theoretical interest relating to age is embodied in the question: "when does a fossil begin?" R. A. Stirton in his recent book, *Time, Life and Man* (1959, page 18) presents one answer in the sentence: "Fossils are the remains or traces of any recognizable organic structures preserved since prehistoric time." Apart from any controversy regarding the meaning of the word "prehistoric," it is clear that Stirton requires that an appreciable period, longer in any case than a human lifetime, must elapse subsequent to the disappearance of the organism as a living entity, before any residue of the organism can be called a fossil. Literally interpreted this would mean that a human bone derived by way of an earth grave from classical Greece, dated at several hundred years BC, is not a fossil, whereas one taken from a site in central California, dated to the middle of the eighteenth century, is a fossil. Would it not clarify the concept more to think of the beginning of a fossil in terms of the initiation of the process of fossilization? Since the process involves some type of change, it is legitimate to

seek the earliest possible change which can occur in the object, which in the future will be called a fossil. If the mode of preservation is ignored, the inception of fossilization can be looked for in the postmortem reactions which occur immediately upon the death of any living organism. Thus, logically, every corpse is a fossil. Such a statement will be regarded as a *reductio ad absurdum* by most archaeologists and palaeontologists, yet it is an unavoidable conclusion if one accepts the premise that fossilization is a dynamic process, moving in time, and involving change of form and turnover of substance. The *rate* of the process, slowed to millions of years or accelerated to a second, is irrelevant with regard to the point in time at which the process takes its inception.

Clearly a compromise is needed and common sense can provide it. An object may be recognized as a fossil if the intrinsic changes have proceeded far enough to be recognized by simple visual methods, and the elapsed time is known to be long enough to be consistent with the changes. Differences of opinion will always exist and will have to be argued in each individual case.

A subsidiary problem relates to which portions of an organism should be regarded as fossil or subject to fossilization. If the concept of uninterrupted postmortem change is adhered to rigidly, irrespective of rates of change, the blood and soft tissues of an animal will have to be regarded as fossil, for with the cessation of the heart beat anaerobic metabolites begin to accumulate and the distribution of electrolytes begins to shift. Then follows the disintegration of form which is called decomposition, usually assisted by bacterial action, but not necessarily so. By common consent this sequence of events occurring in the soft parts is not considered fossilization. There are left, therefore, the hard and resistant structures, bone, teeth and shell, which form substantially all the fossils with which the investigator is called upon to deal. Subsequent discussion will be restricted to these materials.

Condition is the end state of a fossil to which it has arrived through a period of time up to the minute of discovery. It, therefore, furnishes an index to the processes which have been going on in the fossil, that is to the nature and degree of the change, or *alteration* which has taken place.

The term alteration is here defined as the sum of the physical, chemical, and biological reactions which have brought the residual structure to the condition in which the field or laboratory observation finds it. In a word alteration *is* the process of fossilization.

The converse, or antithesis of alteration, is preservation. Preservation implies failure to change or become altered and, of course, the relationship in quantitative sense is reciprocal: the degree of alteration is an inverse function of the degree of

preservation. At one limit there could be conceived a fossil of great age with no alteration whatever. Here preservation would be complete, or one hundred percent. At the opposite limit alteration would be so complete as to have destroyed the fossil as a recognizable entity, and preservation would be *nil*, or zero percent.

The equilibrium, or balance between fossilization, i.e. alteration, and preservation is, like the thermodynamic state of a physical or chemical system, independent of the pathway by which its observed position is attained. Appreciation of this principle will obviate much confusion. Thus heat, dryness, deep freezing, disinfection, and encasement in asphalt or amber are all *types* of preservation, but the end result is identical: the retardation or virtual cessation of alteration. Nor is the *method* of alteration a critical factor in the appraisal of the equilibrium between alteration, or fossilization, and preservation. Bacterial invasion, leaching by soil water, and solution by acid are three possible methods of alteration of a bone. Yet under proper circumstances all three may be equally restrained by a single type of preservation, for example deep freezing.

Although the equilibrium position between alteration and preservation may be in theory independent of pathway, nevertheless it is precisely the pathway which often is of greatest interest to the student of human or animal fossils.

There are two possible pathways, or perhaps highways, of alteration. One involves the loss of substance, or *depletion* of material. This is Leakey's "fossilization." The loss may embrace any component of the living bone, but for simplicity all constituents may be grouped merely as water, organic matter, and inorganic matter.

The second pathway is the reverse of the first, that is the acquisition, or *accumulation*, of material. Leakey and many other archaeologists, palaeontologists and geologists call it "mineralization," although accumulation is by no means confined to mineral, or inorganic matter.

Since these two opposing processes may operate both simultaneously and consecutively, clearly there may be great diversity in direction and rate of movement, as well as in the nature and quantity of all the substances concerned. That is, there may be almost an infinity of pathways whereby a bone has arrived at its present state. Some of the phenomena associated with the alteration of bone and other residues of living organisms are sufficiently common and impressive to merit special recognition.

Thinking in terms of bone, teeth, and shell, depletion may be referred primarily to the three major chemical constituents, water, the mineral framework, hydroxyapatite or its equivalent, and the organic matter, a combination of collagen and mucopolysaccarides or their equivalent.

Water is found in fresh, air-dried bone to the extent of approximately fifteen percent by weight. In fossil bone the content is always reduced, absolutely and relatively, regardless of the momentary environmental water level. Indeed water content provides a rough index to degree of fossilization. The organic matter normally disintegrates and disappears from bone and teeth within a geologically short time. The rate of disappearance, however, varies within wide limits and is contingent upon the physical and chemical characteristics of the environment. Of these, moisture and temperature are perhaps the most important, since they control the velocity of the reactions whereby substances decompose and recombine. The loss of protein, as measured by the reduction in nitrogen content, has been recognized for a great many years, but is strongly influenced by the factors mentioned. Indeed it is dangerous to use nitrogen as an index to the age of a fossil unless the climatic conditions are given full consideration.

A more recent development has been the segregation of amino acids by means of modern chromatographic methods. The native protein gradually hydrolizes with the liberation of the constituent amino acids. Some of these persist for very long periods, and furnish a possible dating clue, as well as being of value in the identification of bone protein in long extinct species. Ultimately, to be sure, even the amino acids are leached out and disappear.

The inorganic framework of hard tissues, hydroxyapatite, is stable and highly resistant to decomposition and destruction. It persists for millions of years and may be detected by light polarization or x-ray diffraction methods in fossils dating at least as far back as the beginning of the Tertiary era. Nevertheless it is probable that hydroxyapatite undergoes some type of very slow alteration. There may conceivably be a reordering of the crystal lattice, and it is not unreasonable to postulate ion exchange mechanisms whereby the apatite molecule might be rendered relatively soluble and thus subject to removal by leaching. On the other hand, it is likely that the rapid disintegration and total disappearance of whole bones observed in hot, wet soils is due to the rapid hydrolysis of the protein with concomitant disorganization of the structure at the microscopic level and dispersion through the soil of the more resistant apatite fraction.

Apart from moisture and temperature the most important influence in determining type and rate of depletion is the chemical composition of the soil or burial matrix, and the most conspicuous single factor is the acidity, or the hydrogen ion concentration. The hydrogen ion accelerates two critical processes. It acts as a catalyst to protein hydrolysis, and quickly, in the historical or geological sense, converts collagen and allied proteins to their constituent building stones such as the soluble peptones and amino acids. At the same time it substitutes for other

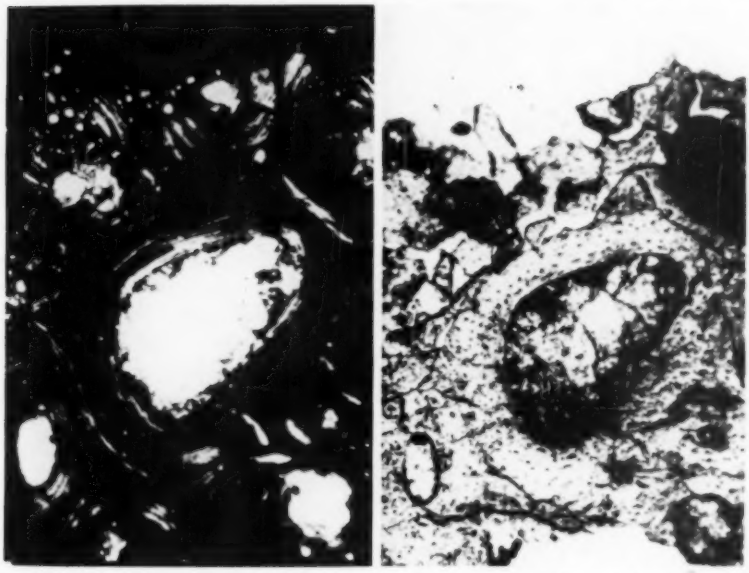
cations in the apatite lattice thereby facilitating the break-up of the crystal. Although this process is normally much slower than the hydrolytic destruction of protein, nevertheless it proceeds with appreciable velocity in acid soils. Extremely high concentrations of acid, as is well known to histologists, will "decalcify" bone in a few minutes. Other ionic species found in the soil are significant with respect to accumulation and replacement in fossils, but are little if at all effective in causing outright depletion.

Accumulation is the deposition in bone of any substance at a level or concentration higher than that found while the bone was in the living state. There is no restriction as to the nature of the material. It may be water, any organic compound, or any inorganic ion or molecule, regardless of whether or not the substance is normally found in the living tissue. Hence "mineralization" is a special case of accumulation, and should not be considered as synonymous with "fossilization."

Accumulation as a process may be considered in several aspects. First there is the *method* of accumulation, second, there is the *degree*, or extent, and third, there is the *nature* of the substance accumulated. These aspects may be separated for the purposes of discussion but in reality they are intimately interrelated.

Accumulation of material necessarily implies the existence of space into which the material may be moved. All animal hard tissues are penetrated by a varying number of lacunae, channels, and canals, which in life serve for the occupancy of cells and other sub-visible structures, and also provide a pathway for the movement of fluids and the transport of materials. After decomposition of the flesh these spaces become vacant, except for water, if the ambient environment is sufficiently saturated with moisture. A great deal of empty space is therefore immediately available. Subsequently new space may be provided only through the removal of some preëxisting structure, and this process is likely to be slow.

Material may move into the bone in any of the three primary physical states, solid, liquid, or gas. Discounting the third as without significance in the present connotation, we find that solid, particulate matter can enter the interstices of bone and remain there. Clay or even fine silt is frequently observed as filling the Haversian canals, and indeed the canaliculi of vertebrate bone. Plate 1 illustrates observations on a thin section from a dinosaur bone. To the naked eye and under a hand lens the bone appears to be perfectly preserved as to shape and internal structure. Under the ordinary microscope and under polarized light, however, it is evident that the whole interior of the bone consists of masses of fine rock fragments in a heterogeneous array of native crystals. These aggregates had slowly intruded themselves as such into the bone, filling vacant spaces, without disturbing the superficial appearance. In principle an analogous condition is seen



Left: Thin slice of bone of *Brontosaurus* photographed under ordinary light microscope. × 62.

Right: The same section photographed under polarized light. × 62.

when cellular organisms such as bacteria or fungi penetrate a fossil, whether by passive transport or active growth. It is proposed that this method of accumulation be designated *massive*.

By contrast most accumulation occurs by translocation of substances in solution. All spaces, of whatever size, in a fossil are filled with air in the completely dry state. On wetting, water moves in to displace the air and carries with it all dissolved material derived from the soil or other matrix. If the bone again dries out or for any other reason the solution becomes supersaturated, the solutes will be deposited or precipitated. Since soil solutions are extremely dilute with respect to most components, the tendency will be for deposition to take place on both macro- and microsurfaces. Thus by a process of accretion the cavities lined by these surfaces will become filled with solid matter, either in the crystalline or the amorphous condition. The rate and extent of such precipitation or deposition depend, to be sure, upon the length of exposure, the amount of solution, and the concentration of the solutes. Since, in general, the process involves the laying down of matter in its ultimate state of subdivision, it is suggested that this method of accumulation be designated *molecular*.

The degree of accumulation may vary from none at all before the entrance of any material, to complete, when all potential space has been filled by foreign substance. A newly killed, freshly buried bone or tooth would represent the former condition and a thoroughly silicified piece of wood the latter. In between, any condition whatever may exist.

The term mineralization refers explicitly to the accumulation of inorganic solutes. The process has started when the first layer of deposit can be detected on the walls of cavities. It is approaching its conclusion when to the sight and touch the fossil is packed solidly, or nearly so, with the intrusive substance. This state is commonly referred to as *permineralization*, a word the meaning of which is discussed by Stirton (1959, page 20). In the extreme it becomes *petrifaction*. It should be borne in mind, however, that at least in theory a bone or other fossil can be completely impregnated with an organic substance, such as tar, in which case another terminology would have to be employed.

The nature of the substances accumulating in bony fossils is highly diverse, and there are many special problems associated with individual materials. Of the organic accumulators perhaps the most universal is humus, or the brown colored, soluble components of humus. The discoloration is usually on the periphery of the bone and is best observed in osseous remains of relatively recent burial. Penetration of tar or asphalt, as at La Brea ("pickling," Stirton aptly calls it) is highly

spectacular and is beautifully illustrative of the preëmption of vacant space by an organic compound.

A host of mineral or inorganic substances can and do accumulate in bone. Some of these are recognized as particularly significant. Iron salts, as a rule in the form of hydrated oxides, are frequently noted. The actual quantity of iron is, however, likely to be relatively small. The brown color makes it conspicuous. Silica and lime salts are perhaps the most important accumulating materials from the quantitative standpoint. Silicon, either as silica (quartz) or as silicates (clay minerals) is extremely abundant in the soil, indeed almost invariably present in the environment of a fossil, and in spite of low solubilities tends to accumulate universally. As deposited it may show either a crystalline or an amorphous structure.

Calcium is always present, and indeed is an essential component of bone itself. In acid soils the free calcium will remain in the ionic form, and although it may exchange with other ions in the bone, it cannot be said to accumulate to an appreciable extent. In soils on the alkaline side of neutrality, particularly those derived from limestone, calcium exists principally as the carbonate, and will accumulate as such in the fossil. The most common form is calcite, but complexes are frequent which include calcium in combination with varying proportions of carbonate and phosphate. One of these is collophane, the exact composition and mode of formation of which has not yet been satisfactorily established. It is known to occur naturally as a derivative of certain phosphatic rocks, but whether it is introduced into and incorporated as such in the fine structure of fossil bone is subject to question. It is entirely possible that what has been called collophane in fossil bone is the product of reactions involving extrinsic calcium and the intrinsic carbonate and phosphate which form part of the apatite crystal. The whole question needs further investigation.

The two primary pathways of alteration are depletion and accumulation. To them must be added a third phenomenon, which represents a combination of the other two. This is replacement, or substitution. To achieve this, two consecutive steps are essential. The first is a loss of original substance, leaving vacant space. The second is the gain of new material, which occupies this space. At the same time, however, there is retention of form, as observed with the eye, or with the light microscope, or even with the electron microscope.

Substitution can occur on three levels of magnitude. The first is relatively gross, and is illustrated by the loss of free water from the interstitial spaces of a bone and their subsequent filling by new material such as might be deposited from the soil solution. The second is intermediate, but sub-microscopic. Minute spaces are cleared by the decomposition of organic micellae of colloidal dimen-

sions, or even apatite crystals dislodged by the disintegration of adjacent structural elements. These vacant spaces can be filled by substitutes contained in the environment. The third is ionic or molecular, the replacement, with a single micro-structural element, of one ion or molecule by another. Here, for example, would be the substitution of a fluorine atom for an hydroxyl group in the apatite lattice, or the exchange of a carbonate for a phosphate ion attached to one of the exposed ultra-surfaces. In short, substitution involves the interplay of all the processes and reactions which may accompany both depletion and accumulation. Nothing fundamentally new has been added.

The alteration of a fossil by any of the pathways discussed is not always consistent and homogeneous throughout the object. Indeed, more often than not in recent fossils is found *differential* alteration. Thus position in the ground may determine whether one surface or another is subject to the action of soil water, or whether the upper portion is more rapidly depleted than the lower. The accumulation of foreign matter almost invariably starts with exterior of a bone and proceeds inward more and more slowly the greater the depth attained. The result is that when disinterred, the bone varies widely in chemical composition from one region to another, particularly in the case of human burials. In extreme instances we find severe depletion in almost direct contact with zones of heavy accumulation, and it becomes very difficult to determine what type of alteration the bone really represents.

Not only chemical but morphological differentiation may occur. The dentine of a tooth may decay, leaving the enamel intact. The spongy epiphysis of a long bone may either disappear, or conversely undergo impregnation with soil solutes, while the compact diaphysis remains substantially unaffected. Even on a micro-scale there may be selective alteration, as for instance the dinosaur bone which, when examined under the microscope, displayed the clear residues of Haversian systems in the midst of masses of intrusive rock particles, all the rest being unrecognizable as osseous tissue.

The problem of the antiquity of an object is too often the predominant question for many investigators and the state of fossilization, mineralization, or preservation is cited immediately as evidence for the assignment of age to a fossil. Not only Leakey, but many other students, have cautioned against the assumption of antiquity on the basis of fossilization. Should age determination be based on the internal evidence of the object under consideration, then an accurate estimate will require first a decision as to what have been the actual pathways of fossilization in the individual instance. Then there must be a judgment concerning the velocity of these processes in the light of the known properties of the soil and climate.

Finally must be considered the probable history of the environment itself. This is a formidable undertaking but each step is essential to a correct solution to the problem of chronological age in the absence of external sources of evidence.

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SYSTEM CHANGE IN SALISH KINSHIP TERMINOLOGIES[1]

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THIS PAPER is concerned with one specific diachronic problem suggested by a major system difference between the kinship terminologies of different Salish-speaking peoples in the Pacific Northwest. The problem may be defined generally as: given certain comparative evidence, can we infer anything definite as to the nature and direction of change in the several Salish terminologies?

The question seems open to approach from both ethnological and linguistic directions. Consequently, evidence bearing on this problem is sought through comparative analysis of Salish terminologies as *systems of term usage* and as *linguistically related morpheme sets*. I have restricted both types of comparison to grandparent-grandchild and uncle-aunt-nephew-niece term sets. The reason for this restriction is that term usage in these classes of relations seems to illustrate most clearly the basic system difference referred to above. Finally, results of the systemic and linguistic comparisons are applied as independent lines of evidence to suggest a most plausible hypothesis as to change processes in Salish kinship usage.

FEATURES OF SALISH KINSHIP

Some preliminary observations on Salish kinship systems will serve to define more exactly the scope of our problem. Two of these are assumptions in part derived from theoretical considerations as to the nature of linguistic and cultural change; the rest are empirical generalizations derived from available descriptive data.

A first assumption, already formulated in general terms by Murdock,[2] is that the clearly demonstrated linguistic relationship of Salishan peoples implies an original Salishan kinship terminology. Lexically, this terminology would have been part of the proto-Salish language; ethnologically, part of the social culture of the proto-Salish language community.

A second assumption, derived in part from data presented below, is that whatever type of kinship terminology characterized the proto-Salish speech community, this original system has developed into two distinct types among modern Salish peoples, and these types correlate with a series of linguistic and cultural differences.

Despite considerable differences in term usage, modern Salish kin-term systems

1 A preliminary draft of this paper was read at the Northwest Anthropological Conference, University of British Columbia, April, 1961. I am indebted to Wayne Suttles and Allan H. Smith for permission to use or refer to unpublished field notes.

2 Murdock, 1949, pp. 346-347.

share certain common features.[3] These may be summarized as: (1) All ParSib are distinguished from Par, and SibChi from own Chi.[4] (2) ParSib and ParPar terms are extended to same generation collaterals of those relatives, and similarly for SibChi and ChiChi (i.e. ParSib = ParParSibChi, ParPar = ParParSib, etc.). (3) Sibling terms are extended to cousins (Sib = ParSibChi), although one or two systems include a special supplementary reference term for "cousin" (e.g., Spokane). (4) Special status-change terms for ParSib and SibChi applied following death of a connecting relative (Par or Sib) are very widespread. (5) Terminology for lineal relatives beyond the second ascending or descending generation (e.g., ParParPar, ChiChiChi) tends to be "generation" in type; often a single term applies reciprocally to members of both generations. It may be added that Salish reference terms are in general not distinct in morpheme form or pattern of usage from terms used in address. We may assume that these universally shared features characterized proto-Salish kinship terminology, and represent stable retentions from that original system of terms.

Apart from common features, Salish peoples of the present are divided as to kinship terminologies into two main sets: (1) Those with *lineal* systems of terms for ParSib and ParPar.[5] (2) Those with *bifurcate collateral* systems in designating these relatives. Lineal terminology does not distinguish ParSib or ParPar according to the sex of the connecting relative; thus, all FaSib = MoSib, all FaPar = MoPar. Bifurcate collateral terminology does so distinguish these sets of relatives; thus FaSib ≠ MoSib, FaPar ≠ MoPar.[6]

This twofold division of lineal and bifurcate collateral systems coincides in general with the linguistic division between Salish languages of the Bella Coola, Coast Salish, and Oregon divisions on the one hand and of the Interior division on the other.[7] It also coincides generally with the ethnographic division between

3 Cf. Spier, 1925, p. 74, for a somewhat different formulation of common Salish kinship traits.

4 Symbols and abbreviations for kin relations are basically those of Murdock, 1949, with some additions and slight modifications. Hu, Wi, So, Da, Fa, Mo, Br, Si, El (elder), Yo (younger) are as in Murdock. I introduce 3-letter terms for sex-generalized relations: Spo (spouse), Chi (child), Par (parent), Sib (sibling); also Dec (deceased), Ma (male), Fe (female), Mn (man's), Wm woman's). Other symbols, representing relations between terms are: - (hyphen), linking two or more terms defining a term set, e.g., ParPar-ChiChi terms: ~ "reciprocal with"; = "same as"; ≠ "different from"; ≅ "same as reciprocal."

5 Spier, 1925, p. 74, defines this system as "Salish type." Some Interior Salish systems, as we shall see, do not accord with the criteria of this type, but fit better his "Mackenzie Basin" or "Eskimo types (*idem*, pp. 76, 79).

6 Lowie, 1928, and Kirchhoff, 1932, originally defined these types independently; see also Murdock, 1949, pp. 141-142, for usage of this system of classification.

7 Classification of the Salish stock in these four main linguistic divisions, on comparative lexical evidence, is presented in Swadesh, 1950.

Salish tribes of Northwest Coast and Plateau culture types. In general, the Bella Coola, Coast Salish, and Oregon (Tillamook) linguistic divisions fall into the Northwest Coast culture area, and show kinship terminologies of lineal type, while the Interior Salish linguistic division falls into the Plateau culture area, and shows terminologies of bifurcate collateral type. The Thompson are an exception to this generalization. Culturally they are a Plateau people, and linguistically Interior Salish, but their ParSib and ParPar terms are lineal. They are, of course, an interior group territorially adjacent to Coast Salish peoples.

Both Salish types of terminology, lineal and bifurcate collateral, occur among other, neighboring, non-Salish peoples. Lineal terminologies characterize Wakashan and Chemakuan as well as Salish peoples on the Pacific coast, and others to the south, including the Yurok in northwestern California.[8] Bifurcate collateral terminologies occur widely among non-Salish groups east of the coastal ranges, including Sahaptins and Upper Chinook (Wishram).[9] Thus, the lineal versus bifurcate collateral division seems to correlate primarily with a culture-type division of Northwest Coast and Plateau rather than simply with a linguistic division within the Salish stock. These facts suggest that diffusional factors, or cultural changes associated with the development of regional social-culture differences, may be among the ultimate causes of the split in Salish terminologies.

HYPOTHESES OF CHANGE

Three logical possibilities occur as to the direction of this Salish development. (1) The original proto-Salish terminology was of neither lineal nor bifurcate collateral type, but developed into these two types in coastal and interior regions, respectively, as a result of linguistic and cultural separation of the proto-Salish speech community. Under this view, coastal and interior systems would represent independent innovations in these two areas.

(2) Proto-Salish terminology was of lineal type, which has been preserved among coastal groups, while Interior Salish peoples have innovated in developing bifurcate collateral terminologies. This view restricts major system innovation to the interior terminologies.

(3) Proto-Salish terminology was bifurcate collateral in type. This system has been preserved among Interior Salish tribes, while coastal groups have innovated in developing lineal terminologies. In this view major innovation was restricted to coastal systems.

Considering the three possibilities above as mutually exclusive hypotheses, it

8 See Spier, 1925, p. 74 and Plate 1, and Kroeber, 1934.

9 Jacobs, 1932; Spier and Sapir, 1930, pp. 262-266.

is the purpose of the ensuing discussion to show that a combination of comparative linguistic and systemic evidence demonstrates greatest plausibility for the third interpretation: that coastal lineal terminologies have undergone major innovation, and that they have been derived from an original bifurcate collateral system.

We dismiss the first hypothesis mainly for lack of any clear supporting evidence. It is, of course, not to be assumed that only one of the divergent systems, coastal or interior, has undergone developmental change. This is in fact directly contradicted by significant variations among Interior Salish groups in their ParSib-SibChi usage. The comparative results presented below do suggest, however, that coastal systems have changed most, and in a particular direction, while interior systems have remained relatively conservative and hence approximate more closely to an original hypothetical proto-Salish system. The second hypothesis, the converse of the third, seems directly contradicted by the results which support the latter hypothesis.

GRANDPARENT-GRANDCHILD TERMS: SYSTEMIC FEATURES

Let us first consider ParPar-ChiChi terms, since these form somewhat simpler and more manageable systems and lead to clearer conclusions than do Salish ParSib-SibChi terms. There are usable data for ten coastal and four interior systems: Bella Coola, Squamish, Musqueam, Nooksack, Lummi, Klallam, Twana, Puyallup, Quinault, and Tillamook (Siletz) on the coast; and Thompson, Southern Okanagon, Spokane, and Wenatchee-Columbia in the interior.[10] Systemic results of a comparison of these terminologies may be summarized as follows.

(1) All coastal groups, and Thompson, agree in differentiating ParPar and ChiChi. All these, except Tillamook, use a single term for the ChiChi relation. All, except the South Georgia groups (Squamish, Musqueam, Lummi, Klallam, Nooksack), use two terms for ParPar (ParFa, ParMo). The South Georgia

10 Sources are: McIlwraith, 1948, vol. 1, pp. 150-156 (Bella Coola); Wayne Suttles, unpublished field notes (Squamish, Nooksack, Lummi); Elmendorf and Suttles, 1960, pp. 20-21 (Musqueam); Gunther, 1927, pp. 258-260 (Klallam); Elmendorf, 1946, pp. 422-424 (Twana—two errata in this source should be noted: p. 424, relations 3 and 4 should read tcə'c-a·l$_i$'c; p. 427, in first diagram of fig. 2 lines for maternal grandparents should be transposed); Smith, 1940, pp. 173-178 (Puyallup); Olson, 1936, pp. 90-92 (Quinault); Frachtenberg, 1917, pp. 45-46 (Tillamook, Siletz dialect); Wayne Suttles, unpublished field notes (Thompson); Walters, 1938, pp. 88-90 (Southern Okanagon); W. W. Elmendorf, unpublished field notes (Wenatchee-Columbia, Spokane). Spokane terminology was compared with that of the Kalispel in field notes of Allan H. Smith; the two are virtually identical. The same appears true of the Flathead terms given by Krueger, 1961. I have modernized the orthography of native terms in the older sources in some instances, for consistency. Transcription is in accord with modern Americanist usage and follows in general the prescriptions of George Herzog et al., *Some Orthographic Recommendations* (American Anthropologist, vol. 36, pp. 629-631, 1934).

groups, forming a linguistically closely related branch of the Salish stock, have a single ParPar term.[11]

(2) The three Interior Salish systems—Southern Okanagon, Spokane, Wenatchee-Columbia—show four ParPar terms, differentiated according to sex of the referent and sex of the connecting relative; thus, FaFa, FaMo, MoFa, MoMo. Further, these terms are used reciprocally for descending-generation relatives; thus, FaFa ≅ MnSoChi, FaMo ≅ WmSoChi, MoFa ≅ MnDaChi, MoMo ≅ WmDaChi. The system results in the descending generation relatives being distinguished not by own sex but by sex of the speaker and sex of the connecting relative. Spokane has an additional self-reciprocal status-change term to designate ParPar ≅ ChiChi after death of the connecting relative.

If the coastal system has been derived from one like that of the interior, then its major innovative changes must have been: (1) Reducation of ParPar terms from four to two (or one), by disregarding sex of connecting relative as a differentiating factor; (2) restriction of ParPar terms to the ascending generation only, by abolishing generation reciprocity; (3) assigning a single new term (except in Tillamook) to the ChiChi relation.[12] Such a system would retain only part of the lexical stock of the original system.

If the coastal system were the result of a single innovative change, then we should expect the modern coastal terminologies to agree, in a lexical comparison, with *a particular part* of the interior term sets; the other original terms would have been lost to the coastal languages at one innovative stroke, so to speak. On the other hand, if the coastal system has resulted from multiple parallel innovations undergone independently by earlier forms of different Coast Salish languages, then modern coastal terminologies could be expected to agree lexically with *various parts* of the interior term sets. This last condition would result from survival of different terms from the original set in different independently innovating coastal systems.

GRANDPARENT-GRANDCHILD TERMS: LINGUISTIC FEATURES

These two derivative hypotheses can be tested by linguistic comparison. We first note, for the three interior bifurcate collateral terminologies, that three (and possibly four) of the four Wenatchee-Columbia ParPar terms are apparent cog-

11 For the South Georgia branch as a special linguistic grouping within the Coast Salish division, see Swadesh, 1950.

12 The new coastal ChiChi term was apparently innovative, at least in that usage, although it also appears in Thompson (and Shuswap). Tillamook (Siletz) usage for their two ChiChi terms is hard to make out from Frachtenberg's definitions, but this system lacked the morpheme used for ChiChi in other coastal systems.

nates of corresponding Spokane terms, while Wenatchee-Columbia and Spokane share two cognates in this set with Southern Okanagon.

Turning to the coastal terms, we note the following cognate correspondences with terms for which at least two cognates can be found in interior languages.

(1) Coastal (ParFa): BC kukpi, Puy sápa, Quin čopə, Til xɛha$^{?}$s. Interior (FaFa): SOk sxáxba, Spok sx̣ápɛ, WC sx̣əx̣ápə.

(2) Coastal (ParFa, ParPar): Twa sí·la (ParFa); Squa sé$^{?}$la, Mus sí$^{?}$la, Nook síl$^{?}$ai, Lum síla, Klal síya (all ParPar). Interior (MoFa): Spok sʃ́·la, WC stətí·la (WC possibly a noncognate analogical loan from Sahaptin).

(3) Coastal (ParMo): BC kik^{y}a, Twa káya, Puy kaíya, Quin či (and Thom kié). Interior (MoMo): Spok ččiyé, WC kəkíya. (Initial k- in the Twana and Puyallup words involves a phonological difficulty).

(4) Coastal (ParMo): Til qéna$^{?}$s. Interior (FaMo): SOk káqəna, Spok qán·a, WC qəqán·a.

(5) Coastal (ChiChi): BC slemc (apparently s-l-emc, cf. emc SibChi), Squa íməc, Mus $^{?}$íməθ, Nook íməθ, Lum $^{?}$íŋəs, Klal íŋəc, Twa íbac, Puy ébac, Quin émac (also, Thom íməč, Shuswap émc). Interior: Nil, except possibly SOk snémat "inclusive term for all grandchildren," apparently supplementary to generation-reciprocal use of ParPar terms. This seems, like the Bella Coola term, a derivative; the hypothetical stem *-emat does not occur as a kinship term (in Walters' material), unlike BC slemc ChiChi, emc SibChi.

(6) Coastal ("ChiChi"): Til síla$^{?}$s. Interior (MoFa ≅ MnDaChi): see 2, above.

(7) Coastal ("ChiSo/Da"): Til číya$^{?}$s. Interior (MoMo ≅ WmDaChi): see 3, above.

The correspondence may be tabulated as follows:

Interior	*Coastal*	*Coastal Systems*
FaFa	→ ParFa	BC, Puy, Quin, Til
FaMo	→ ParMo	Til
MoFa	→ ParFa	Twa; So. Georgia groups (→ ParPar)
MoMo	→ ParMo	BC, Twa, Puy, Quin (also Thom)
MoFa	→ "ChiChi"	Til
MoMo	→ "ChiSo/Da"	Til
(nil)	ChiChi	All (except Til), and Thom

These results are in striking agreement with the assumption of derivation of coastal systems from an original system like that of interior groups, and of multiple independent innovations in coastal systems, all leading to lineal terminologies.

If the assumed direction of derivation is reversed, an apparently impossible situation results. We note that all of the coastal-interior cognates appear in the Spokane ParPar-ChiChi term set. Then, an interior system such as the Spokane would have resulted from the lexical amalgamation of several different term sets, which is hard to credit. This consideration leads to dismissal of the second hypothesis, referred to above (p. 367)

Three main patterns of derivation appear from these data: (1) FaFa → ParFa, MoMo → ParMo; (2) MoFa → ParFa (→ ParPar, So. Georgia groups), MoMo → ParMo (→ zero, So. Georgia groups); (3) FaFa → ParFa, FaMo → ParMo, MoFa and MoMo (generation-reciprocal) → ChiChi terms. The first and second patterns involve restriction of original generation-reciprocal usage to older generation only, with a new ChiChi term supplied. The third pattern also restricts original generation reciprocity, in male-side terms to older generation, in female-side terms to younger; and of course, like the other patterns, it removes the male-side and female-side distinction.

The first pattern is shared by the Bella Coola with Puyallup and Quinault, but doubtless represents two independent innovations, in Bella Coola and southern Coast Salish division languages respectively. Intervening Coast Salish terminologies show the second pattern. The third pattern is restricted to the Tillamook.

Our analysis thus far indicates that Salish kinship system changes have taken place in accordance with the hypothesis that major innovation has been restricted to coastal systems. We conclude that coastal lineal grandparent terminologies were formed by reduction-selection from an original four-term bifurcate collateral system, such as that of the Wenatchee-Columbia, Spokane, and Southern Okanagon. Further, reductions of the original system seem to have been effected independently in different coastal groups, since linguistic cognates of the coastal ParPar terms appear among all four of the interior terms. The coastal system is basically the same throughout, except for further reduction to a single ParPar term in groups of the South Georgia linguistic branch; however, it has not been arrived at by any single innovative selection of terms. Instead, at least four separate innovations seem to have taken place—among the ancestors of the Bella Coola, the northern coastal division tribes (south to the Twana), the southern coastal division tribes (Puget Sound and Olympic branches), and the Tillamook.

It is noteworthy that, while the linguistic innovations have been multiple, the systemic results are consistently similar. All coastal systems have reduced the number of ParPar terms to two or one; all have restricted their usage to the ascending generation (where the original term set was apparently generation reciprocal,

applying also to ChiChi); and all but the Tillamook have supplied a new term for ChiChi (in Bella Coola a derivative of the term for SibChi). Tillamook is unique in having restricted original female-side terms (MoFa ≅ MnDaChi and MoMo ≅ WmDaChi) to the younger generation.

The only consistent feature in these coastal system changes seems to be sex of the designated relative (in the ascending generation); that is, FaFa and MoFa terms are preserved as ParFa, FaMo and MoMo terms as ParMo. Even here the South Georgia groups are an exception in their special generalizing of an original MoFa term to usage as ParPar. We may assume, somewhat speculatively, that this special South Georgia development followed an earlier two-term innovation in which original MoFa and MoMo were generalized as ParFa and ParMo, and the latter term was then lost.

UNCLES, AUNTS, NEPHEWS, NIECES: SYSTEMIC FEATURES

One's first impression on examining Salish kin terms for the ParSib-SibChi class of relatives is that these show the same clearcut distinction between coastal lineal and interior bifurcate collateral usages as do the ParPar-ChiChi terms. Closer analysis discloses greater complication and more special problems than arise in a comparison of ParPar-ChiChi terms. Salish ParSib-SibChi term sets are more variable, both systematically and linguistically, than are the term sets for ParPar-ChiChi relations. It may be assumed that they resulted from more numerous and extensive innovative processes than in the case of the grandparent-grandchild terms. Yet the general results of a comparison of ParSib-SibChi terminologies are in agreement with the conclusions derived above from analysis of ParPar-ChiChi terms.

We find the following five patterns of usage for ParSib-SibChi terms among the ten coastal systems examined and the Thompson, who here, as in ParPar-ChiChi usage, agree in general with the coastal systems.

(1) ParBr, ParSi, SibChi (Bella Coola, Squamish, Puyallup, Quinault).
(2) ParSib, SibChi (Musqueam, Nooksack, Lummi, Klallam).
(3) ParBr, ParSi, MnSibChi, WmSibChi (Twana).
(4) ParBr, ParSi, SibSo, SibDa (Thompson).
(5) FaBr, MoBr, ParSi, BrChi, SiChi (Tillamook).

The most widely distributed coastal system has three terms as listed as no. 1 above, in which Bellâ Coola agrees with several other linguistically diverse groups. Four groups of the South Georgia linguistic branch (Musqueam, Nooksack, Lummi, Klallam) have reduced distinctions in this class to one of generation only, in accord with their general tendency to generation terminology, as in ParPar-

ChiChi terms. Klallam has essentially the same system as the others, but introduces a distinction of ParElSib and ParYoSib, and designates their younger-generation reciprocals as YoSibChi and ElSibChi, respectively; the latter two terms are obvious derivatives of the same stem. It is not difficult to derive this South Georgia system from usage like no. 1 above, as we have already done in the case of the South Georgia ParPar-ChiChi terms.

Twana agrees with the most general coastal system (no. 1), except in introducing a unique distinction according to sex of speaker in SibChi terms. Thompson also has two SibChi terms, but distinguishes these according to own sex.

Only Tillamook distinguish ParSib by means of bifurcation, and then only with reference to males; nieces and nephews are not distinguished by own sex, but by sex of the connecting relative, a kind of bifurcation. The system is not like that of other coastal tribes, but, like these, can apparently, be derived from an original system similar to those of the Southern Okanagon, Spokane, and Wenatchee-Columbia.[13]

In addition to the term usages listed, most coastal systems have also two status-change terms (DecParSib, DecSibChi); Tillamook has one ("relative after death of connecting relative").

In the three Interior Salish systems—Southern Okanagon, Wenatchee-Columbia, and Spokane—ParSib terms are consistently bifurcate collateral. All three distinguish ParSib from Par, and in addition distinguish these relatives by sex of the Par and by own sex, resulting in a four-term system (FaBr, FaSi, MoBr, MoSi). Spokane in addition distinguishes MnFaSi and WmFaSi. Thus, in comparison with interior usage the systems of coastal groups show the same pattern of reduction of number of terms in the ParSib category as in that of ParPar. In both categories Thompson, as noted, goes with coastal usage.

Descending-generation usage is very different, not only between coastal and interior systems, but among the latter. Southern Okanagon has a single SibChi term, and generation-reciprocity does not, apparently, apply to the ParSib terms. Wenatchee-Columbia shows four terms for SibChi relations, MnBrChi, WmBrChi, MnSiChi, WmSiChi, all apparently distinct from Par-Sib terms. Spokane has a complex system in which the principal term distinctions are: MnFaBr = MnBrSo, FaBr = MnBrChi (partially overlapping the first term in meaning), MnFaSi = WmBrSo, WmFaSi = WmBrDa, MoBr, MoSi, MnSiChi, WmSiSo, WmSiDa.

13 A not dissimilar system for ParSib-SibChi relations appears in Siuslaw (Lower Umpqua dialect); see L. J. Frachtenberg, "Siuslawan (Lower Umpqua)," in Franz Boas, ed., *Handbook of American Indian Languages, Part 2* (Bureau of American Ethnology, Bulletin 40, 1922), pp. 461-462.

Spokane has in addition two status-change terms, DecParSib and DecSibChi. Southern Okanagon shows one such term, DecParSib; the material used has no item for DecSibChi, though one suspects some special term usage in this case. My Wenatchee-Columbia data likewise show no status-change terms in the ParSib-SibChi class, though this may be due to failure to record such designations.

It may be observed of the Spokane system that no terms refer to both paternal and maternal relatives, that all paternal terms are generation-reciprocal, and that maternal and female-side terms form a different system in which there is no generation-reciprocity and sex of speaker distinguishes terms only in younger-generation referents.

The diversity in descending-generation usage, within the ParSib-SibChi class of terms, among these three Interior Salish systems is striking. The three tribes concerned speak fairly closely related languages, especially close in the case of Southern Okanagon and Spokane (whose SibChi usages differ most markedly), and are probably very similar in their social systems. Obviously innovative change has strongly affected these interior systems in their ParSib-SibChi usages, so that no one of them can be taken as representative of original proto-Salish usage in this class of relations. Yet they do all show a common feature, bifurcate collateral usage in ParSib designations, which contrasts systemically with coastal usage.

UNCLES, AUNTS, NEPHEWS, NIECES: LINGUISTIC FEATURES

In determining directions and patterns of change between interior and coastal systems, it would be helpful if we could arrive at some conclusions regarding the relative conservatism of the three interior term sets designating ParSib-SibChi relations. If, for example, we could arrive at evidence suggesting that one of the interior systems was closer to or more typically representative of proto-Salish usage than the other two, this would assist in placing interior-coastal comparisons on a sounder foundation and would enable us to draw surer diachronic conclusions from such comparison.

Linguistic comparison of Southern Okanagon, Spokane, and Wenatchee-Columbia terms for ParSib-SibChi relations does seem to provide such evidence. In the data tabulation on page 375 terms cognate among the three systems are arranged horizontally on the same lines.

In cognate relationships, Spokane is obviously the most interconnected of the three systems, while Southern Okanagon and Wenatchee-Columbia are less connected with each other than is either with Spokane. Further, the Southern Okanagon and Wenatchee-Columbia connections include no cognate relationships not also shared by both these systems with Spokane. Spokane also shows a meaningful

Southern Okanagon	*Spokane*	*Wenatchee-Columbia*
	ɬpúsəmən MnFaBr ≅ MnBrSo	
smíɬ FaBr	səmɛ́ʔɬ FaBr ≅ MnBrChi	səmʔɛ́l FaBr
sḱóqʷɛʔ[14] FaSi	sḱúkʷi MnFaSi ≅WmBrSo	
staták^{w}a FaSi	täˑtík^{w}ə WmFaSi ≅ WmBrDa	tík^{w}a FaSi
səsí MoBr	sısí MoBr	
		qása MoBr
swawása MoSi		
	qáxa MoSi	qáxa MoSi
	túˑnš MnSiChi	túˑnx MnSiChi
	sk^{w}səʔɛ́lt[15] WmSiSo	sq^{w}əˑsə[16] WmSiChi
	stˑmʔčɛʔɛ́lt WmSiDa	
łuístən DecParSib	łuwéstın DecParSib	
słəłwílt SibChi	słuwɛ́lt DecSibChi	słuwɛ́lt MnBrChi

14 Apparently the Northern Okanagon term, used as an alternative to staták^{w}a at Tonasket. The cognate Spokane term looks like a reduplicative diminutive of sk'úi (MnMo).

15 This and the term below contain the derivative suffix -elt, "child, young one." Cf. Spok sk^{w}sɛ́ʔ (So), stˑmčɛʔɛ́lt (Da); the term for WmSiDa is apparently formed from that for Da by a glottal-stop infix, of possibly diminutive import. The forms were carefully checked phonetically.

16 WC WmBrChi was attested by informants as a separate term, but the form is uncertain.

distinction of terms for MnFaSi and WmFaSi which are apparently lumped as synonyms for FaSi in Southern Okanagon.[17]

We can derive several immediate conclusions from these observations. The system differences in SibChi usage, in particular, indicate that innovations have occurred in all three systems, but probably most extensively in Southern Okanagon and Wenatchee-Columbia. Linguistic considerations make it apparent that the innovations are not shared by any two of the three systems, and that the Spokane ParSib-SibChi terminology is the most likely to have been least affected by innovation; it is therefore relatively conservative within this group of three Interior Salish systems. We can further conclude that the Southern Okanagon and Wenatchee-Columbia terminologies have been independently affected by innovations which can be roughly characterized as (a) term reduction, and (b) restriction of reciprocal generation usage.

If coastal ParSib-SibChi terminologies have been derived from an original system perhaps best represented by recent Spokane ParSib-SibChi usage, then these same innovative processes must have played a major role in the formation of the coastal systems. However, linguistic evidence indicates again that the innovations were adopted separately in different coastal groups.

Linguistically, the terms for ParSib-SibChi relations in coastal Salish groups are extremely diverse, much more so than the reasonable uniformity of system usage for these terms would lead one to suppose. Cognate terms for this class among coastal languages, with interior cognates noted where apparent, seem to be limited to the following cases.

(1) Coastal (ParBr): BC sisi, Squa sése. Interior (MoBr): SOk səsí, Spok sɪsíʔ.

(2) Coastal (ParBr): Twa kásiʔ, Puy kasé, Til qɛ́saʔs (MoBr). Interior (MoBr): WC qása. (The k-q correspondence makes this comparison doubtful for Twana and Puyallup, unless k- is misrecorded for q-).

(3) Coastal (FaBr): Til swaáls. Interior (FaBr): SOk, smíɬ, Spok səmɛ́ʔɬ, WC səmʔɛ́ɬ.

(4) Coastal (ParSib): Lum séčs, Klal cáčc (ParElSib), Til čac (ParSi). Interior (ElSib): Spok ɬčíčšɛ (WmElSi), WC čáka (MnElSi). (The Spokane cognate is doubtful; it may represent a reduplicated stem *či-čšɛ plus ɬ- prefix, and thus cognate with Wenatchee-Columbia kə́ʼx (WmElSi) and Twana čə́ʼš (ElSi).

(5) Coastal (SibChi): Squa stáeɬ, Twa stáʻyʔɬ (DecSibChi). Puy stálał

17 Or perhaps represent Northern and Southern Okanagon terms, used as synonyms by some Southern Okanagon.

(SibChi) and Twa stalát (WmSibChi) are possibly connected. No interior cognates are apparent.

(6) Coastal (SibChi): Mus stíwən, Nook, stíwən, Lum stík^{w}ən, Klal stíq^{w}in (ElSibChi). Interior (FaSi): SOk staták^{w}a, WC ták^{w}a, Spok tă·ták^{w}ə (WmFaSi ≅ WmBrDa). The Spokane generation-reciprocal usage suggests that the other interior systems and coastal usage have here developed in different directions by different restrictions of generation reciprocity.

(7) Coastal (SibChi): Twa słuá·laš (MnSibChi), Til słɬgálš (BrChi). Interior (SibChi): SOk słəɬwílt, WC słuwélt (MnBrChi), Spok słuwélt (DecSibChi). (As noted previously, the element -ɛlt (etc.) in the interior terms is a suffix denoting "child, young one").

(8) Coastal (DecSibChi): Squa swɪnəmáɬ, Mus swənméyɬ, Nook swɛnimáiƛ', Lum sk^{w}ənŋíčƛ. All are languages of the South Georgia branch. No interior cognates appear.

(9) Coastal (DecParSib): Lum ḱsɪčaɬ, Klal skésačaił (DecSibChi). Note reversal of generation-reciprocity. No interior cognates.

(10) Coastal (DecParSib): Squa sa^{ʔ}ɑx^{w}ɬ, Quin six^{w}áɬ (SibChi; cf. síx^{w}ən, DecParSib). Interior: Spok scíx^{w}əlt (Chi, ChiChi, lineal descendant; səx^{w}síx^{w}əlt, "children and grandchildren").

(11) Coastal (SibChi): BC emc; cf. BC slemc ChiChi, and other coastal (and Thompson, Shuswap) ChiChi terms cited above, p. 370.

We should also note that while the Thompson terms for ParBr and ParSi are difficult to find cognates for, their terms for SibChi have clear interior but no coastal cognates: Thom sk^{w}əšɛ́ (SibSo), Spok sk^{w}əsɛ^{ʔ} (So), sk^{w}sə^{ʔ}ɛ́lt (WmSiSo), WC sq^{w}ə'sə (WmSiChi); Thom sƛ'əmqéit (SibDa), Spok st'mčɛ^{ʔ}ɛ́lt (Da), st'm^{ʔ}čɛ^{ʔ}ɛ́lt (WmSiDa). Thus, Thompson is terminologically more like other interior languages, despite its agreement in system of usage with the coast. This suggests that the accordance of Thompson usage with the coastal lineal system, both in ParPar-ChiChi and in ParSib-SibChi terms, is the result of contact influence from coastal cultures, and thus represents a special Thompson innovation due to this factor.

Patterns of cognate correspondence in the above material are not as clear as in the case of ParPar-ChiChi terms, yet in so far as we can define them they do not seem to be at variance with the conclusions regarding derivation of coastal systems arrived at from analysis of the ParPar-ChiChi terminologies. The following patterns are apparent in ParSib-SibChi terms; in each case the interior application is cited first, followed by the meaning or meanings in coastal systems.

(1) MoBr → ParBr. (2) MoBr (another term) → ParBr. (3) FaBr → FaBr.

(4) ElSi → ParSib (Si). (5) FaSi → SibChi. (6) SibChi → SibChi. (7) Chi (etc.) → DecParSib, SibChi.[18]

There are indications in the list of a change in generation reference among coastal terminologies. Pattern no. 5, for example, shows a cognate correspondence of Southern Okanagon and Wenatchee-Columbia terms for FaSi (and a cognate term of broader meaning) with coastal (South Georgia branch) terms for SibChi. This can be explained by assuming, as we have done on other grounds, that Spokane generation-reciprocal usage is here original (WmFaSi ≅ WmBrDa). Usage in the other two interior systems, on the one hand, and in the South Georgia coastal systems, on the other, would then represent secondary restrictions of generation reciprocity to ascending and descending relatives, respectively. Indications of an old generation-reciprocal usage may also appear in pattern no. 7.

Three of the four Interior Salish ParSib terms show cognate correspondences in coastal terminologies—patterns 1, 2, 3, and 5 above. Meanings or range of application are usually different, but only the interior terms for MoSi seem to lack cognates in coastal terms. We thus have a situation comparable to the coast-interior relations in ParPar terms, which can be interpreted plausibly only by assuming derivation of coastal systems from something more like the interior systems.

The linguistic data on ParSib-SibChi terms shows a high degree of noncomparability of terms in this set between the various coastal terminologies. Most of such cognates as appear are shared by adjacent groups or those closely related linguistically. On the other hand, the number of distinct terms in this class is generally less in any coastal system than in the three interior systems with bifurcate collateral arrangement.

We infer that if present coastal ParSib-SibChi systems were all (or most of them) reflexes of a single original Coast Salish innovation which reduced an original proto-Salish system from bifurcate collateral to lineal terminology, then these systems would show higher cognate ratios in their term sets than is actually the case. But, since the coastal systems are lexically quite heterogeneous, it appears more reasonable to conclude that their reduction from an original bifurcate collateral terminology was the result of a number of independent innovative changes. All of these independent changes brought about the same systemic results: reduction of the number of terms in the ParSib-SibChi set, and (except in Tillamook) consistently lineal term usage. However, since these changes went on independently in different earlier coastal systems, the linguistic results were lexical diversity and a considerable number of noncognates among the different terminologies.

18 The symbols used express both cognate correspondences, for terms of designated meaning, and hypothetical direction of derivation.

Innovative change has in this case operated more extensively on the coast than in ParPar-ChiChi term sets, yet the same pattern of diverse correspondence in cognates with different parts of the interior term sets appears as in the case of the ParPar-ChiChi terms. Thus, consideration of ParSib-SibChi terminologies does not contradict but rather strengthens the conclusions derived from analysis of ParPar-ChiChi terms.

GENERAL CONCLUSIONS

The principal findings and conclusions of this study may be indicated in general summary.

(1) Major innovative change in Salish kinship terminologies has affected coastal systems, including such linguistically diverse divisions of the Salish stock as Bella Coola, Coast Salish, and Tillamook. These peoples have developed varieties of lineal terminology in the ParPar-ChiChi and ParSib-SibChi classes of relations.

(2) Coastal lineal terminologies have been derived from an original bifurcate collateral terminology similar to systems found among some Interior Salish, and perhaps best exemplified by the Spokane, through two main processes: reduction of number of distinctive terms through generalizing meanings; and, restriction of generation-reciprocity by distinguishing ascending from descending generation usage. Certain common features of Salish kinship usage have been retained in both systems, coastal and interior.

(3) Linguistic comparison indicates derivation of coastal terminologies not only in the direction indicated above—bifurcate collateral to lineal—but through multiple independent but parallel innovations.

FURTHER PROBLEMS SUGGESTED

Granting the applicability of these conclusions, a number of problems respecting kinship usage may be settled or illuminated in new ways.

First, we may enquire if both the systems examined for Salish peoples—coastal lineal and interior bifurcate collateral—show common features of Spier's "Salish system."[19] Or, put another way, are all Salish peoples properly to be lumped as having a single type of kinship terminology? The common features of all or most Salish systems cited above are impressive, but equally striking are the divergences in methods of classifying ascending-generation relatives shown by coastal and

19 Spier, 1925, p. 74 and Plate 1. Among the defining features of the "Salish type" are: merging of father's and mother's siblings; one term for "uncle" and one for "aunt"; one term for nephew or niece; terms for "grandparent," "child," "grandchild." These criteria fit most coastal systems, but not those of the interior (other than Thompson).

interior groups. In his Salish type Spier included as defining criteria certain common Salish kinship features and others referring only to lineal terminologies. This Salish type was held to include Coast Salish, a number of other coastal non-Salish groups, and the Interior Salish Lillooet, Shuswap, and "possibly" Spokane. The Lillooet and Shuswap may turn out to go with coastal peoples—their interior British Columbia neighbors the Thompson certainly do—but we must remove from this category not only the Spokane, but probably all southerly Interior Salish, of the Wenatchee-Columbia, Okanagon, Spokane-Kalispel-Flathead, and Coeur d'Alêne groups. It is not implied that Spier intended to restrict his Salish type to Salish peoples (as he obviously did not), nor has he categorically included in it all Salish groups.

Secondly, coastal and interior systems of Salish kin terms appear to present an example of two "bilateral" systems with a different number of term distinctions, the interior systems having more for the classes of relations examined (ParPar-ChiChi and ParSib-SibChi). Murdock has presented an hypothesis according to which "distinctions in kinship terminology should appear in association with relatively insignificant functional differences between kinsmen more often in bilateral than in unilinear societies."[20] Coast Salish and Interior Salish societies are both bilateral, yet the latter make more distinctions in term usage than do the former. It is not impossible that a greater number of "insignificant functional differences" between kinsmen appear in Interior Salish societies than in those of coastal tribes, yet I know of no clear-cut ethnographic data which would lead us to this conclusion. This is a perhaps profitable direction of future research.

A third problem involves the question of whether the lineal terminology of coastal Salish groups is an ancient and conservative system, retained perhaps from some remote proto-Mosan-Algonkian past, and therefore shared by such linguistically and geographically discrete peoples as Wakashans, Salish, and Yurok. Kroeber seemed to think that such was the case.[21] If the results of the present study have been interpreted correctly, then we must abandon this view, at least so far as Salish lineal systems are concerned, since these can be explained as innovative within the framework of Salish data alone, and do not require reference to historically wider (or deeper) linguistic units.

A much more general and perhaps more significant problem has to do with whether there are any causal or influencing factors which can be shown to correlate with the particular line of change in Salish kinship terminologies defined in this study. It is not proposed to explore this problem here, but there do seem to be

20 Murdock, 1949, p. 110.

21 Kroeber, 1960, pp. 347-348 (fn.); also, 1934.

several promising lines of investigation which might shed light on the wider question of kinship systems as indicators of organizational level or degree of social development.

One striking difference in social culture between coastal and interior Salish tribes is the prevalence of ranked status distinctions in the former groups. This is, of course, a general distinction between Northwest Coast and Plateau societies, and applies to peoples on the coast other than Salish. We have already noted, however, that Coast Salish lineal terminology is shared with other coastal peoples; there is therefore a correlation with some aspect or aspects of coastal culture.

It is not perhaps easy at first glance to see how this distinction between Northwest Coast and Plateau types of social structure could reflect itself in the coastal reduction of kin term distinctions. Yet something along this line could probably be worked out in terms of a general theory of evolution in kinship systems recently proposed by Service.[22] I was struck by the fact that his formulation seems to fit well such a specific case as we have here: the smaller number of terminological distinctions for immediate kin and the presumably more complex structure of general status relationships in coastal cultures, vis-à-vis those of the Plateau area. Service's notion of "devolution" or progressive simplification of "egocentric-familistic" systems of nomenclature in more complex societies is suggestive and worth pursuing in this connection.

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THE DEATH PRACTICES AND ESCHATOLOGY OF THE KIOWA APACHE[1]

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BECAUSE of the excessive fear of the ghost among the Apachean-speaking tribes of the Southwest and Southern Plains, death is a traumatic experience for them, one which focuses attention on all possible allies of dark forces and which evokes elaborate ritual defenses. Understandably, therefore, it is an area of culture rich in interesting concepts and symbolism. As a result it has received a good deal of notice in the literature and has been the subject of a number of studies.[2] This paper is meant to add to that series and to help pave the way for a comparative study that it is hoped will throw light upon the history, differentiation, and interrelations of the Apacheans.

The Kiowa Apache believed that the ghosts of dead relatives came to conduct the dying to the afterworld. A seriously ill person might mutter: "They come after me; I've got to go. I've lived pretty poor on this earth. My mother, my father, come after me."[3] That these visits of the ghosts of the departed are associated with death and ill fortune is indicated by the kind of appeal addressed to the corpse as soon as death occurred: "Don't you ever turn your face to us." "Don't look back, keep on going; when I get old and die I will see you." "If you look back evil will come upon us."[4]

Among the Kiowa Apache the reaction to a death was immediate and violent. Close relatives wailed, tore their clothes, and exposed their bodies without shame; some shaved the head, lacerated the body, and cut off a finger joint. Women made a greater demonstration of grief than men, and the spouse of the dead person was expected to mourn most ostentatiously of all. A widow might dress in a garment made of smoke-blackened material from old tipi flaps, and she remained in this abject condition until relatives of her deceased husband, usually his sisters, re-

1 The senior author was engaged in bringing together the scattered material for this article when he asked Dr Bittle to check some Kiowa Apache terms for him. Dr Bittle sent him not only the correct transcription of the terms but valuable unpublished material on the death complex that he had gathered in the field. Dr Bittle further volunteered to do additional field work to fill in the data and to clarify difficult points, and gathered most important information on two subsequent occasions. He gave so much time to the project and his contribution was so significant that a joint article was suggested.

2 For these studies see the bibliography.

3 McAllister, 1935, p. 121.

4 *Ibid.*

leased her from some of the mourning obligations and dressed her in good clothes. Similarly a widower went to great lengths to show his grief. Sometimes he requested relatives of his wife to gash his forehead or to cut off all his hair and gave presents to the one who performed the service.[5]

The corpse, and particularly the skull, was considered a source of danger and contamination; the word for skull symbolizes not only the corpse but all the material and polluting aspects of death.[6] It was believed hazardous even to get too close to the dead. Fainting spells, insanity, and facial paralysis could result from such exposure or contact. Consequently the body was prepared and disposed of with all dispatch. A close relative, usually a woman, washed the body with yucca suds, combed the hair, and painted the face, often with red and yellow ochre. Occasionally an outsider or a nonrelative was induced to prepare the body and was paid handsomely for this disagreeable task. The dead person was dressed in his best clothes. If death occurred during the day, the funeral took place before sundown. If the death occurred at night, the body was disposed of the following day. A prompt burial reduced the possibility that the ghost would linger and "talk" to relatives. The doorway of the Kiowa Apache tipi opened to the east; but because "the doorway is for the living," the corpse was removed from the north, west, or south side. Usually only close relatives followed the travois and made up the burial party. If someone who was not a kinsman attended, he was given a gift. Whenever possible, the corpse was placed in a shallow, rocky crevice in hilly country. Burial near streams was avoided for fear that the body would wash out. The personal belongings of an individual, such as his weapons and shield, might be buried with him. One eye-witness account mentions that the body of a Kiowa Apache chief was interred face downward, but this may have been happenstance.[7] The favorite horse of the deceased was killed at the site of the grave by piercing it in the neck or shooting it in the head. As the animal collapsed, the wife of the dead man might grasp its neck, become spattered with its blood, and fall to the ground with it. In the case of a chief or prominent man, a number of animals might be sacrificed. On one occasion two horses and a mule were dispatched at the

5 The practice of lacerating the body at the death of a kinsman is mirrored in one of the coyote tales. Turkey tricks the coyotes into killing their youngest child. The story continues: "Coyote ran out of the camp, and rushed into a patch of briars. He let the briars cut him all up to punish himself. When he did this, he set an example for the Indians. Now, when a loved one dies, the Indians cut themselves with knives, and sometimes chop off their fingers, just so they can hurt themselves. It makes them feel better" (Bittle, 1956).

6 The Kiowa Apache term for owl is closely related linguistically to the word for skull. The fitness of this will be apparent when the position of the owl in the death complex and its connection with the ghost are discussed.

7 Yarrow, 1881, p. 142.

grave of a Kiowa Apache chief.[8] Rocks and thorny branches were placed on top of the grave to discourage marauding animals. After the burial the site of a grave was strictly avoided.

Upon the return of the funeral party any remaining property of the deceased was destroyed. Most of the possessions which were combustible were burned; even the ordinary tipi was destroyed, though heraldic tipis were preserved and passed on to close male kin. A warrior's designed shield was buried with him or destroyed, but the right to make a similar shield was inherited by the nearest male relative. Surplus clothes of the dead person were not burned but were thrown into a river; ". . . you gave them to the river for some future return."[9] Apparently there was some vague idea that the property disposed of at the time of death would be of use in the after-life. However, any property or ceremonial knowledge a person had given to others before his death could be retained. The personal ceremonial equipment of the deceased was destroyed, but ritual objects of tribal importance, such as medicine bundles, were inherited in the male line or, in the absence of male kin, were placed in the custody of female relatives. If a man owned many horses, those which were not killed at his grave became the honored and pampered property of his brother. Their manes were trimmed and their tails were cropped, and they were ridden only to battle.[10] If they perished in war, it was considered a credit to the deceased. Relatives of the dead wailed and mourned anew when they saw these horses about the camp and made a particular show of grief if any of them failed to return from an encounter.

Members of the burial party changed clothing upon their return. Those who had not cut their bodies washed with water. Those who had gashed their bodies rubbed fat on the cuts and could not wash until the wounds were healed. The mourners rubbed themselves liberally with sage. All close kinsmen of the dead person and those who had been involved in the funeral rites fumigated themselves freely with smoke from fires in which cedar, sage, or the root of a plant called "medicine fat"—powerful prophylactics against ghosts—were burned. In fact, sage, cedar, and "medicine fat" were used whenever the fear of ghosts or thoughts and dreams about the dead aroused anxieties. To ward off hazards associated with death and ghosts, a cross of cedar ash was traced on the forehead, also. Death rendered a locality unclean and dangerous. If a family in which a death occurred had been camping with others, the entire group moved from the place where the

8 *Idem*, p. 143.

9 Bittle, 1959.

10 Because of this practice and its associations, the manes and tails of the horses of the living were never trimmed.

person had died. The immediate family of the deceased stayed at some distance from the others for a while. During this period of semi-isolation the mourners would go to elevated spots to wail and there would be visited by friends who sought to comfort them.

Not only was the memory of the dead person suppressed by the destruction of his property and of objects associated with him, but there could not be any direct reference to him. His name could not be mentioned, particularly in the presence of his relatives, and words that were similar to or identical with his name could not be uttered. Persons who had used a certain kinship term for the deceased now addressed those of similar relationship with secondary terms. Because of the strictness and extensions of these usages, names were not conferred upon children until it was fairly certain that they would survive.

The elaborateness of practice and the intensity of anxiety in respect to death varied with the age and status of the deceased. The whole group engaged in mourning for a chief; but because of their innocence and lack of rancor, the bodies and ghosts of children caused little concern, and much the same can be said about very old people who had lived their life out to the full. Nor was the slain enemy feared. Explained an informant: "You don't have to worry about being hurt by the ghost of an enemy. When you kill someone in battle, he is totally eliminated. There is nothing left, not even his ghost."[11] It was the corpses and ghosts of Kiowa Apache who had died in the prime of life which caused most worry to the living.

The most usual method of disposal of the dead was interment, but in the case of warriors scaffold burial also was practiced. There is the possibility that lodge or tipi burial, at least of a temporary nature, occasionally occurred, too. An informant asserted that the body of a prominent man was sometimes left for a day or two in a tipi before final disposal in the hope that he would be restored to life.[12] Two stories have been collected from the Kiowa Apache which include descriptions of tipi burial.[13]

The close relatives of the deceased, particularly the brothers of a dead man and the sisters of a dead woman, not only mourned energetically but were careful to see that the memory of their dead kinsman was treated with proper respect. If the dead man belonged to one of the male dancing societies, the organization had to obtain consent from his closest male relatives before it could hold its next function. If it failed to do this, the outraged brother, father, or uncle could invade the place of meeting, destroy the drum and other property, and prevent the dance from continuing. The organization would then have to mollify the relatives with gifts

11 Bittle, 1959.

12 *Ibid.*

13 McAllister, 1949, pp. 93, 120.

and to smoke and mourn for the departed. If some of the members of a war party were slain, the victory dance was postponed out of respect for the dead and regard for the feelings of the bereaved. The blood kin of the deceased kept a close watch on his spouse to make sure that mourning obligations were not taken lightly. If this person had been a good husband or wife and was well liked by the family, the burden could be lightened and shortened; but unmistakable evidence of grief was ordinarily expected for an initial period. The close blood kin of the dead person also expected to have a good deal to say about the future marriage arrangements of a surviving widow or widower. There was a strong feeling in favor of sororate-levirate arrangements. If a woman who died had an unmarried sister, it was she who ordinarily took care of any children left motherless by the death. She was also the leading candidate for the hand of the widower, once the mourning period was over. In fact, the mourning period could be shortened if a marriage between the women and her dead sister's husband was agreed on. If the deceased woman had no eligible sister, real or classificatory, the widower still had to wait at least a year before he could make marriage arrangements for himself independently, had to present gifts to his relatives-in-law, and had to gain their consent. If he acted too hastily or without permission, his wife's sisters had the right to beat him and his new wife without interference. On the other hand, if he acted with due respect, even though he ultimately married outside his former wife's family, his ties with its members remained strong. The affinal terms they had used to each other were continued, and his new wife was treated with courtesy and addressed by a kin term. The same principle held if it was a man who died. Then his wife was expected to mourn ostentatiously and in time to wed an unmarried brother of her deceased husband, if his family so desired. In fact, this man was obligated to look after the welfare of his dead brother's wife and children, and the marriage was essentially a sharpening and formalizing of the responsibility. In case there was no marriageable brother-in-law, if a woman honored the memory of her dead husband long enough and obtained permission for an outside alliance, the former ties with her relatives-in-law were retained and even extended to her new husband. On the other hand, any lack of seriousness in mourning or a marriage outside the family without permission was resented and brought swift reprisals. The brothers of the dead man could then beat the woman or cut off the tip of her nose and also destroy the property of the man she had married. In addition, a supernatural punishment menaced a widow or widower who married without permission before the mourning period ended. Such a person and his mate were likely to be persecuted by the affronted ghost of the deceased. Sometimes the relatives

of a person who had become the target of the ire of a dead man's kin would try to pacify them by inducing them to smoke with them and forgive the couple.

The burial and mourning practices which have been summarized are best understood in terms of the concepts which enter into Kiowa Apache eschatology. There are three main concepts of this kind to keep in mind: a vital force or animating spirit which continues as a neutral or harmless spirit after death; "evil tendencies" which develop during life and continue on in association with the ghost in afterlife; and the ghost which comes into being at death and is a potential source of danger to the living thereafter. The "evil tendencies" do not appear in personified form; rather, they are the background and dynamics of the ghost's activities. It is the ghost which assumes definite shapes and which uses recognized instrumentalities to achieve its dark ends.

The life principle or animating spirit in man is called dà·ɣą́·. Dying can be described by saying, "Dà·ɣą́·" is fading," or "Dà·ɣą́·" is going." We shall arbitrarily refer to dà·ɣą́· as "spirit." At death the spirit leaves the body for an afterworld, usually considered to be sky world or heaven. The spirits of all human beings, regardless of whether they have been good or evil on earth, have the same destination. The spirits of those whose conduct was questionable in life are purified and capable only of good in heaven. Spirits do not interfere in earthly affairs and cannot assume the form of animals or natural forces. They remain in their sky world and enjoy a perpetual existence of abundance and pleasure. Kiowa Apache who participated in the Ghost Dance Religion and who fell down in trance have claimed, on their recovery, to have been in heaven and seen departed relatives there. Obviously the living had no sense of persecution by these spirits.

Less pleasant and much less easy to label by a single term is the notion of bà'é·h. Linguistically the form is probably related to the word for "gray." Semantically the matter is more complex. Bà'é·h causes the individual to do destructive and antisocial acts in anger. It is involved in feats that show power and magical immunity but about which there is something unsavory and weird. It is absent in the newborn child, and an exceptionally good person may be the repository of very little of it at the end of his life. The conduct of a woman who marries too soon after the death of her husband is explained by bà'é·h, and so is the behavior of the shaman who traffics with owls and ghosts to his own advantage. Perhaps the closest we can come to conceptualizing bà'é·h, is to think of it as the evil tendencies, powers, and impulses that accumulate during life. Bà'é·h enters Kiowa Apache eschatology because death does not terminate it. It is as much a component of the ghost as it is of the nature of the living adult; and it is, of course, far more danger-

ous and uncontrollable in the ghost. These precipitates of human malice, perpetuated beyond the grave, take no separate shape and are not seen or felt as such. But they furnish the motive power for ghosts which seek to return to the encampments of the people and spread terror and sickness.

Whereas the "evil tendencies" in an individual have their origin in life and continue into the after-life, the čį'yé or "ghost" (literally, the word conveys the meaning of "to be no good") comes into existence only at death. The proper destination of the ghost is an afterworld, generally conceived of as an underworld. It, too, is a place of "no scarcity" and of comfort. McAllister was told that it is located under a mythological northern lake from which one of the important medicine bundles was obtained. A Kiowa Apache who entered the lake and secured the ceremony and bundle saw the ghosts of his dead friends and relatives there and, while sitting in a tipi, "he heard the ordinary sounds of camp life—the murmuring gossip of women, the scraping of hides, and the barking of dogs."[14]

As long as ghosts are content to travel to their afterworld and remain there, they do no injury to living men. But they tend to resist the termination of their life: they linger about the corpse, the place of burial, their former haunts and possessions; and, even when they do go to the afterworld, they have the power to return to persecute or harass those with whom they have scores to settle. No ghost comes back except on frightening errands; no ghost is seen or heard by a human being without a sense of terror and despair. The least violent act a ghost can perform is to lead a new shade to the afterworld. Even this is interpreted by the living as a threat; the ghosts are constantly trying to extinguish life, to draw the living to the dead.

Although children who died were not feared, their very helplessness and vulnerability to supernatural attack made them ready targets of ghosts and particularly susceptible to evil influences associated with death. Consequently it was important to keep children from seeing or touching a corpse or from watching funeral processions. Because ghosts sometimes appear as whirlwinds or as sparks (lightning?) that move ahead of whirlwinds, children were taught to look away or cover the eyes at the approach of a whirlwind, and the face of a baby was shielded by an older person so he would not see this phenomenon.[15] Also, because of con-

14 McAllister, 1955, pp. 162-163. Bittle has twice recorded accounts of the visit to the underwater world to obtain this medicine bundle, once in 1956 and again in 1959. Brant obtained a version of the same myth in 1949. Neither was told that this was the realm of ghosts, though McAllister, who did his work in the early 1930's, was explicitly informed that this was the case. See Bittle, 1956 and 1959, and Brant, 1949.

15 A Kiowa Apache story about Coyote and Whirlwind ends with these words: "The Indians say, 'You should never fool with a whirlwind. It's dangerous' " (Bittle, 1956).

cern over the ghost's undue interest in children, the footprints of a child who was learning to walk were rubbed out. If an infant had to be left alone, a stick was placed across the cradle at the chest to protect it from ghosts. Older children were trained to ignore whistling noises, for ghosts try to attract attention to themselves by such means.

There are also a number of birds and animals that are associated with death and the ghost. It is Crow who is charged with the origin of death. Angered because he was not given the plumage of the eagle in the early days of the world, he threw a rock into the water, saying, "If this comes up there will be no death, but if it sinks there will be death."[16] Though the crow is unloved for this, there is no evidence of direct association between it and the ghost on this account.

Dogs, coyotes, and wolves are also connected with thoughts and portents of misfortune and death. It is said that the coyote howls before a death occurs and that the howling of a dog or wolf is also warning of death or bad luck. For this reason howling dogs are often driven from camp. Certain animals and birds are sufficiently feared to be used as bugaboos. Children who misbehave are told that the wolf, coyote, or owl will get them unless they mend their ways.

If the ghost alters its appearance, it is as an owl that it is most likely to return. Accordingly, the presence of an owl about the encampment arouses liveliest apprehensions, and the hooting of the owl is interpreted as a threatening message. Since the owl is active in the nighttime, it is believed that attacks by ghosts occur mostly after nightfall. One of the names for ghosts is "they move around at night," probably a reference to the malignant ghost in the form of an owl.[17]

There are shamans with ceremonies to control ghosts and owls and to cure ghost sickness, but it is a question whether these religious practitioners are not ultimately as much feared as the malady they claim to heal. Since they can influence ghosts and owls, they are suspected of practicing witchcraft through them on occasion, of sending owls and ghosts to sicken those who have offended them, or even, for a fee, of dispatching ghosts to trouble someone who has done them no harm. Moreover, it is hinted that some of them use their knowledge first to strike down a fellow tribesman with ghost sickness and then to cure him of it in

16 McAllister, 1949, p. 22.

17 The members of a small but important men's society of the Kiowa Apache used owl feathers on their dance costumes and were expected to react with particular heroism to the simulated hoot of an owl. The members were noted not only for their bravery but for their unusual practices. They used "backward behavior" and "backward language"; and one of their officials, called Owl Man, was permitted extreme license in sexual matters. See McAllister, 1955, pp. 154-155.

exchange for fine presents. These shamans are considered by many to have more than a fair share of "evil tendencies" and are believed to be prominent among those who return as injurious ghosts after death. In their ritual they claim to employ the owl as a messenger between themselves and the dead, and they make much use of feathers of the eagle and the swift-hawk in their ceremonies.

It is interesting to consider from what direction ghost sickness is thought to come and who is most likely to be affected by it. It is agreed that there is not much need to fear the ghost of a good and kindly person, but one must not overlook the possibility of masked or suppressed antagonism, either. Consequently there are few dead persons who are not suspected at all. An undercurrent of fear permeates the whole thought area. Perhaps the most potent curse in the language is to call a person a ghost. If conflict with an individual prior to his death was open and sharp, there is even more cause for alarm, for "if the dead person has a grudge against somebody, the ghost comes back and causes sickness in that person."[18] The close and constant interaction between kin makes it almost inevitable that a good share of the "grudges" and guilt feelings remain within the family circle. As a Kiowa Apache put it: "It bothers mostly the members of the family. . . . I think that the more closely related you are to the dead person, the more the ghost may bother you. My wife always believes this, and she thinks that her father's ghost may come back."[19] Women and children are considered more vulnerable than men to ghost sickness and witchcraft.

Ghost or owl sickness has certain rather standard characteristics of development and symptom. After the death of a relative or friend, a person may be gloomy and depressed. He may dream of the dead and of death; and since such dreams are portents of what may happen, he fumigates generously with sage, cedar, and "medicine fat" and prays that the events pictured in the dreams will not come to pass. He seeks to forget the matter, for thinking and dreaming of the deceased are likely to induce the return of the ghost. If his thoughts and dreams are of a kinsman, the cultural prescriptions place him in an ambivalent, anxiety-provoking position. He destroys the property of his relative and refrains from mentioning his name in order to banish his memory and to prevent the musings that lead to visits from ghosts. Nevertheless, he must remember to play the mourner in regard to dress, social life, and other matters for some time. This alternation of repression and exhibition makes it really impossible to put the dead person out of mind, and his inability to accomplish this feeds his dread of the return of the ghost. The process is circular and self-defeating and may account

18 Bittle, 1959.
19 *Ibid.*

for the excessive amount of anxiety that surrounds the death complex of the Kiowa Apache and other Apachean groups.

If the symptoms are no more severe than depression and bad dreams, the situation is usually handled by means of the common remedies for dealing with ghosts. But an acute case may involve hysteria, insanity, or paralysis, particularly facial paralysis. Then a shamanistic ceremony is required. Ghost or owl sickness, and especially sickness of this kind sent by a witch, usually is marked by a twisted mouth and a "twisting" of the eyes. In fact, ghost sickness is traditionally associated with ailments of the upper part of the body. More recently polio has been attributed to ghost sickness, the affected part being the area touched by the ghost.

Kiowa Apache culture was badly shattered and impoverished by the time the first serious study of it was made by McAllister in 1933-34. There are now only a few members of the Kiowa Apache tribe who can speak the language with any fluency, and there are even fewer who today know much about the older cultural traditions. It is almost certain that details of the Kiowa Apache death practices have been lost which can never be recovered. Yet we can be thankful that we have as good an outline as we do, and we can look forward to using it to good advantage in comparative studies.

For example, even the most cursory review of the materials shows some extremely significant agreements between the Kiowa Apache and the Lipan Apache in respect to practices and beliefs at death. Besides the core of traits that all Apacheans share, such as the fear of ghosts, hasty burial, etc., there are others in which the two tribes tend to correspond closely and contrast with most of the others. For instance, the Lipan, like the Kiowa Apache, make a distinction between the life principle or spirit and the ghost; and they ask the corpse not to look back during its journey to the afterworld. They also shoot the favorite mount of the dead person in the throat and trim the manes and tails of the horses that are spared. They, too, move the whole encampment when a death occurs. Likewise, isolation of the family of the deceased for some time is practiced, and the same custom of the comforting of the bereaved by outsiders is found among them. For both of these tribes juniper is a purifying agent useful against ghosts, and widows and widowers who marry too soon have reason to fear persecution at the hands of the ghost of the former mate. There are many Kiowa Apache parallels to Jicarilla Apache practices, too, which need to be scrutinized; but these do not seem to be quite as impressive as the Kiowa Apache-Lipan resemblances. At any rate, those who are prone to see the Kiowa Apache as an offshoot of the Sarsi or as the remnant of a separate migration into the Plains will have to consider evidence of this kind.

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